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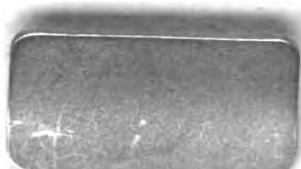
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PHILOSOPHICAL
TRANSACTIONS,
OF THE
ROYAL SOCIETY
OF
LONDON.

VOL. LXXVII. For the Year 1787.

PART I.



LONDON,

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MDCCLXXXVII.

A D V E R T I S E M E N T.

THE Committee appointed by the *Royal Society* to direct the publication of the *Philosophical Transactions*, take this opportunity to acquaint the Public, that it fully appears, as well from the council-books and journals of the Society, as from repeated declarations which have been made in several former *Transactions*; that the printing of them was always, from time to time, the single act of the respective Secretaries, till the Forty-seventh Volume: the Society, as a Body, never interesting themselves any further in their publication, than by occasionally recommending the revival of them to some of their Secretaries, when, from the particular circumstances of their affairs, the *Transactions* had happened for any length of time to be intermitted. And this seems principally to have been done with a view to satisfy the Public, that their usual meetings were then continued for the improvement of knowledge, and benefit of mankind, the great ends of their first institution by the Royal Charters, and which they have ever since steadily pursued.

But the Society being of late years greatly enlarged, and their communications more numerous, it was thought advisable, that a Committee of their members should be appointed to reconsider the papers read before them, and select out of them such, as they should judge most proper for publication in the future *Transactions*; which was accordingly done upon the 26th of March 1752. And the grounds of their choice are, and will continue to be, the importance and singularity of the subjects, or the advantageous manner of treating them; without pretending to answer for the certainty of the facts, or propriety of the reasonings, contained in the several papers so published, which must still rest on the credit or judgment of their respective authors.

It is likewise necessary on this occasion to remark, that it is an established rule of the Society, to which they will always adhere, never to give their opinion, as a Body, upon any subject, either of Nature or Art, that comes before them. And therefore the thanks, which are frequently proposed from the chair, to be given to the authors of such papers as are read at their accustomed meetings, or to the persons through whose hands they receive them, are to be considered in no other light than as a matter of civility, in return for the respect shewn to the Society by those communications. The like also is to be said with regard to the several projects, inventions, and curiosities of various kinds, which are often exhibited to the Society ; the authors whereof, or those who exhibit them, frequently take the liberty to report, and even to certify in the public news-papers, that they have met with the highest applause and approbation. And therefore it is hoped, that no regard will hereafter be paid to such reports, and public notices ; which in some instances have been too lightly credited, to the dishonour of the Society.



C O N T E N T S

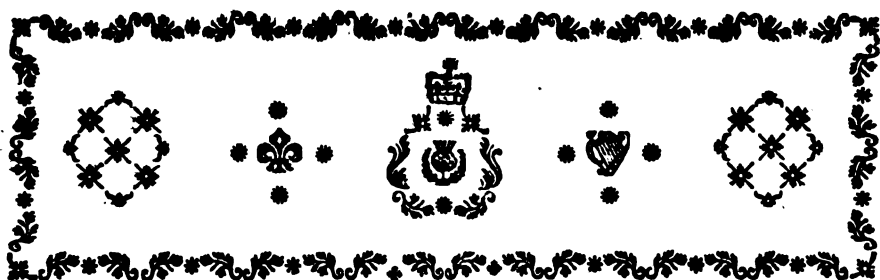
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P H I L O S O P H I C A L
T R A N S A C T I O N S.

I. *An Account of a new Comet. In a Letter from Miss Caroline Herschel to Charles Blagden, M. D. Sec. R. S.*

Read Nov. 9, 1786.

S I R,

IN consequence of the friendship which I know to exist between you and my Brother, I venture to trouble you in his absence with the following imperfect account of a comet.

The employment of writing down the observations, when my Brother uses the 20-feet reflector, does not often allow me time to look at the heavens; but as he is now on a visit to

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Germany,

Germany, I have taken the opportunity of his absence to sweep in the neighbourhood of the sun, in search of comets; and last night, the 1st of August, about 10 o'clock, I found an object very much resembling in colour and brightness the 27th nebula of the *Connoissance des Temps*, with the difference however of being round. I suspected it to be a comet; but a haziness coming on, it was not possible entirely to satisfy myself as to its motion till this evening. I made several drawings of the stars in the field of view with it, and have inclosed a copy of them, with my observations annexed, that you may compare them together.

August 1, 1786, 9 h. 50', the object in the center is like a star out of focus, while the rest are perfectly distinct, and I suspect it to be a comet. Tab. I. fig. 1.

10 h. 33', fig. 2. the suspected comet makes now a perfect isosceles triangle with the two stars *a* and *b*.

11 h. 8', I think the situation of the comet is now as in fig. 3.; but it is so hazy that I cannot sufficiently see the small star *b* to be assured of the motion.

By the naked eye the comet is between the 54th and 53d Ursa majoris, and the 14th, 15th, and 16th Comae Berenices, and makes an obtuse triangle with them, the vertex of which is turned towards the south.

August 2. 10 h. 9', the comet is now, with respect to the stars *a* and *b**, situated as in fig. 4. therefore the motion since last night is evident.

10

* A doubt having arisen about the identity of the stars marked *a* and *b* in the figures, I have examined that part of the heavens in which the comet was the 1st of August, in order to settle this point, but find so many small stars in that neighbourhood that I have not been able to fix on any of them that will exactly

10 h. 30', another considerable star *c* may be taken into the field with it, by placing *a* in the center; when the comet and the other star will both appear in the circumference, as in fig. 5.

These observations were made with a Newtonian fiveepeer of 27 inches focal length, and a power of about 20, the field of view is $2^{\circ} 12'$. I cannot find the stars *a* and *c* in any catalogue; but suppose they may easily be traced in the heavens; whence the situation of the comet, as it was last night at 10 h. 33', may be pretty nearly ascertained.

You will do me the favour of communicating these observations to my brother's astronomical friends.

I have the honour to be, &c.

CAROLINE HERSCHEL.

Slough, near Windfor,
Aug. 2, 1786.

answer these figures; and as they were drawn from observations made by moonlight, twilight, hazy weather, and very near the horizon, it would not be at all surprising if a mistake had been made: however, as these figures were only given with a view to shew the motion of the comet, the conclusion of the change of place, which was drawn from them, was equally good whether these stars were the same or different.

Dec, 14, 1786.

WILLIAM HERSCHEL.



I. Remarks on the new Comet. In a Letter from William Herschel, LL.D. F. R. S. to Charles Blagden, M. D. Sec. R. S.

Read Nov. 16, 1786.

DEAR SIR,

AS my Sister's letter of the 2d of August, relative to the comet discovered by her, has had the honour of being communicated to the Royal Society, I beg leave to add the following remarks upon it.

The track of the parallel not being taken at the time of her observations, I have endeavoured to recover it by means of directing the same instrument which was used on this occasion towards that part of the heavens where it was placed the 1st and 2d of August. Hence, from the annexed figure (see Tab. I. fig. 6.) in which A, B, represents a parallel of declination, we may conclude, that the comet was nearly in the same meridian with the star a ; but more north than it by an interval equal to the distance of the small star b from a . This will consequently give us a pretty good opportunity to ascertain the comet's place with some accuracy.

I have the honour to be, &c.

WILLIAM HERSCHEL.

Slough, near Windsor,

Nov. 15, 1786.

P. S.

P. S. The first view I had of the comet, after my return from Germany, was the 19th of August, when with a 10-foot reflector it appeared not much unlike the third nebula of the *Connoissance des Temps*, with which it might be very conveniently compared on account of its proximity. It was, however, considerably brighter, and seemed to have a very imperfect and confused kind of gathered light about the middle, which could hardly deserve the name of a nucleus. It had also, besides a diffused coma, a very faint, scattered light towards the north following part, extending to about three or four minutes, and losing itself insensibly.



III. *Magnetical Experiments and Observations.*

By Mr. Tiberius Cavallo, F. R. S.

The Lecture founded by the late HENRY BAKER, Esq. F.R.S.

Read Nov. 16, 1786.

THE Bakerian Lecture, which last year I had the honour to deliver to the Royal Society, contained the account of some magnetical experiments, particularly concerning the magnetism of brass, from which it appeared, that most brass becomes magnetic, so far as to attract the magnetic needle, by being hammered, and loses its magnetism by annealing or softening in the fire; but that there is some brass, which possesses no magnetism naturally, nor acquires any by hammering.

Several experiments, made since the reading of that paper, having shewn a few particulars, which tend to correct what was advanced in the said Paper, I shall in the present lecture mention, first, those particulars, and shall then proceed to relate other experiments and observations relating to other branches of the same subject of magnetism.

In performing the experiments on the magnetism of brass, I generally used a magnetic needle suspended in a peculiar manner, as it is described in my last lecture; *viz.* a common sewing needle, or a piece of steel wire rendered magnetic, and
suspended

suspended by means of a chain of hair; which sort of suspension I find not only from the experiments then made, but also by several subsequent trials, to be the nimblest hitherto contrived; because some substances which seem to be quite destitute of magnetism, by not attracting any of the magnetic needles otherwise suspended, will sensibly affect this. However, notwithstanding the nicety of this method for discovering a very low degree of magnetic attraction, it was found still inferior to that of exploring substances floating on the surface of quicksilver, as used by M. BRUGMAN*. It seemed, therefore, necessary to repeat some of those experiments on brass, and also upon platina, by examining their magnetism by this means, *viz.* by putting the pieces of brass or grains of platina upon the surface of quicksilver, and then presenting a strong magnet near them. The result of those experiments was, that very seldom a piece of brass, or grain of platina, occurred, which was not affected by the magnet; and even when they were not affected by it, their indifference, as may be expressed, was not very clear and decisive; and indeed there are very few substances in nature which, when examined by this means, are not in some degree attracted by the magnet, so general is the dispersion of iron, or such is the tendency which most bodies have towards the magnet.

Such brass which in the former experiments appeared to have no magnetism naturally, nor to acquire any by hammering, was now found to be mostly magnetic, though in so very small a degree as to be discoverable only when floating upon quicksilver. The same was the case with the grains of platina before they were hammered; but after hammering:

* See his *Magnetismus, seu de Affinitatibus Magneticis*, printed at Lyons in 1778.

their.

their attraction towards the magnet became more evident; whereas those pieces of brass, which naturally had not any degree of magnetism sufficient to affect the needle, nor acquired any by hammering, but yet shewed some tendency towards the magnet when floating upon quicksilver, never, or very seldom, had that tendency increased by hammering.

As in the course of those experiments it naturally occurred to observe several particulars, which may be of use to those persons who wish to repeat these experiments, I shall now subjoin the principal of them.

It is necessary first of all to observe, that the vessel wherein the quicksilver is put for the purpose of examining the magnetism of divers bodies, must be at least six inches in diameter; otherwise the substances that are set to float upon the mercury, will be continually running towards the sides of the vessel, on account of the curvature of the surface of that metal, which in narrow vessels begins from a greater distance from the edge, than in vessels of a larger diameter.

It is necessary likewise to keep the quicksilver very clean, and also very pure; the want of which precautions will render the event of the experiments precarious. I have observed a very remarkable phenomenon, with respect to the surface of the mercury that is used for this purpose. It is, that though substances will float upon it with wonderful nimbleness, when the mercury is first poured out of the bottle into the open vessel, yet a short time after, *viz.* after having remained for an hour or two, and sometimes for a shorter time, exposed to the atmosphere, a piece of brass or other substance will by no means float upon it with equal facility; so that some pieces of brass, or grains of platina, which, after first pouring the quicksilver into the open vessel, were evidently attracted by the magnet,
about

about an hour after were not in the least attracted by it. The method which, when the surface of the quicksilver is rendered thus sluggish, will effectually purify it, is to pass the quicksilver through a funnel of paper, *viz.* a piece of clean writing paper rolled up conically, and having at its apex an aperture of about a fiftieth part of an inch in diameter; which operation is sometimes necessary even on first pouring out the quicksilver, and which I have often been obliged to repeat three or four times in the course of an hour. There seems to be formed a kind of crust upon the surface of the mercury when exposed, which, though invisible by mere inspection, may be perceived by moving the floating body; for if it be tried immediately after having passed the quicksilver through the paper funnel, the floating substance will seem to proceed by itself; whereas, some time after, the same body, when moved, seems to communicate that motion to the adjacent quicksilver, and to drag it along with itself, somewhat like when one moves a body, which floats upon the surface of a liquor that begins to coagulate.

The formation of this crust seems to be mostly owing to the imperfect metals, which in various quantities are usually amalgamated with the common sort of quicksilver; because that amalgamation tends to dephlogisticate those metals, and the semicalcined part floats at the top, and it is not unlikely that the dephlogistication goes on much quicker in the open air. The reality of this supposition is corroborated by observing, that the purer the quicksilver is, the smaller is the crust formed, or opposition made to the floating bodies. However, I have observed it in some measure even in the purest quicksilver that can be procured; and am inclined to think, that it must be

partly owing to moisture, or to very fine dust that adheres to the quicksilver when exposed to the atmosphere.

In performing such experiments care should be had to keep the air and the quicksilver as much undisturbed as possible, and also to present the magnet in a proper manner, *viz.* so as not to touch the surface of the mercury, nor to strike against the floating body, especially when that is in motion; for after the impulse, though that may be very slight, the floating body will be impelled backwards, which may be often mistaken for magnetic repulsion. The least exceptionable method is the following: first, if the floating body be in motion let it stop, then hold a strong artificial magnet nearly in a perpendicular direction, and with one pole just over one side of the floating body, or rather so that the perpendicular, let fall from the pole of the magnet to the surface of the quicksilver, may be about one-tenth of an inch distant from the body to be tried. The height of the magnet above the quicksilver should be just sufficient to let the floating body pass under it without touching it. In this situation the magnet must be held steady; and if the floating body has any magnetism, it will be soon drawn directly under the magnet.

In these experiments it will be generally found, that one part of the floating body is more magnetic than the rest, which appears from that particular part being constantly drawn directly under the pole of the magnet; whereas, when the magnetism is diffused equably, the center of gravity of the body (provided its shape be not very irregular) becomes stationary just under the pole of the magnet.

It is not every magnet that will discover the very weak magnetism of certain substances; for sometimes a powerful magnet

net will evidently attract what a weaker one will not move in the least.

I shall lastly observe, with respect to the experiments of last year's lecture, that though then I thought to have fused and incorporated together brass and iron, yet some subsequent trials gave reason to believe, that the iron is concealed in some part or other of the melted brass, rather than equably diffused through the substance of the latter; and the principal reason for this suspicion is, that when those pieces of mixed metal are tried upon the quicksilver, some points in their surfaces are generally attracted by the magnet in preference to others.

Experiments and observations relating to the attraction between ferrugineous substances and the magnet, in their different states of existence; to which are added, some thoughts concerning the cause of the variation of the needle.

It is a proposition well established in magnetics, that soft iron, or soft steel, acquires magnetism very easily, and loses it with equal facility; but that hard steel acquires that power with difficulty, and afterwards retains it obstinately. From the consideration of those properties I was led to imagine, that if a piece of steel, whilst red-hot, were placed between magnetic bars, and whilst standing in that situation cold water were to be suddenly poured upon it, so as to harden it, there might, perhaps, be obtained an artificial magnet, much more powerful than what can be produced by the ordinary way; because the magnetic bars, employed for such purpose, would communicate a great degree of magnetic power to the steel, when red hot and consequently soft, which power would be fixed upon the steel by the hardening.

In order to put this project to the trial, six magnetic bars were so disposed in an oblong earthen vessel, as that the north poles of three of them might be opposite the south poles of the three others, forming two parcels of bars, lying in the same direction, and about three inches asunder, which nearly was the length of the steel bar which was intended to be rendered magnetic. Things being thus disposed, the steel bar was made quite red hot, and in that state was placed between the magnetic bars; after which cold water was immediately poured upon it, which rendered it so hard as not to admit being filed: its magnetism was found to be considerably strong, but by no means extraordinary. From repeated trials with steel bars of different sizes, and by using a greater or less number of magnetic bars, I found, that short steel bars acquire a proportionably greater degree of magnetism by this method, than those which are longer; that the magnetism in the longer bars is not proportionably as strong, principally because the artificial magnets being placed at their extremities have very little power on those parts of the pieces of steel which are nearer its center; and, lastly, that when, in order to remedy the just mentioned impediment, more magnets are placed nearer the middle of the steel bar, then this piece of steel generally acquires many successive magnetic poles.

Upon the whole it seems, that though this method alone be not sufficient to communicate to steel bars an extraordinary degree of magnetism, yet it may be of great use in constructing large artificial magnets; for if those bars, instead of being hardened in the usual way by plunging them when red-hot in water, be hardened whilst standing between powerful magnets, they will thereby acquire a considerable degree of magnetic power without any additional trouble to the workmen.

men. They may then be polished, after which they may be rendered more strongly magnetic by the usual method of touching them with other magnetic bars; whereas it is a very laborious operation to render magnetic large bars of hardened steel from the very beginning, *viz.* when they have none of that power.

In the course of performing those trials, I frequently observed, that the pieces of steel, whilst they were red hot, seemed not to be attracted by the magnets; so that the least shock, and even the pouring of the water, could remove them from the proper situation, which rather surprised me; because it has been asserted, by some authors, that the magnet attracts red-hot iron as well as cold. KIRCHER especially says, that he tried the experiment *, and found that a piece of iron heated so as to be hardly discerned from a burning coal, was attracted by the magnet as easily as when cold; and he even assigns a reason why the power of a magnet is destroyed by a great degree of heat; whereas the red heating of the iron will not prevent its being attracted by the magnet. The reason he gives is, that the fire corrupts and calcines the magnet, but purifies the iron. The following experiments were made in order to ascertain this matter.

I kept a piece of steel in the fire till it was quite red hot, and in that state presented the magnet to it, so as to touch it repeatedly in various places; but no sign of attraction could be perceived before the redness disappeared. I mean, however, such redness as may be evidently seen in the clear daylight; for, as was shewn by other experiments, when the magnet begins to attract the heated iron, the redness of the latter can still be seen in the dark.

* De Magnete, lib. I. p. II. theorem xxxi.

Having

Having repeated the experiment with different pieces of iron and of steel, the result was constantly the same, *viz.* whilst the iron or steel remained quite red hot or white hot, the magnet did not attract it; but the attraction began when that degree of redness, which is clearly perceivable in the daylight, began to disappear; and it was as strong as ever when the iron was cooled a little more than when the redness quite disappeared in the dark. In regard to this limit or maximum of attraction, I think I have observed, as well as the nature of the experiments would permit, a difference between steel and iron, which is, that in the steel the maximum of attraction follows the disappearance of the red heat sooner than in iron.

This experiment is subject to two sources of mistake, which perhaps misled Father KIRCHER, and which are necessary to be mentioned for the sake of others who wish to repeat this experiment. The first is, that when a piece of iron of no great extent is red hot, or even white hot, in one place, and below a red heat in other parts, the magnet will frequently attract it, though the red-hot side be presented to it. The second cause of mistake is, that when a small piece of iron or steel, as a common sewing needle, is made red-hot, and is then presented to the magnet, if the magnet touch it, that contact cools it instantly below the necessary degree of heat, and of course the attraction takes place. It is owing to this last cause that I have not yet been able to ascertain, whether the attraction between the magnet and the iron be quite annihilated, or only diminished to a great degree, by rendering the iron red or white hot; so that I can only say with certainty, that a magnet will not attract a certain piece of iron red hot or white hot; whereas it will attract another piece of iron at least fifty times bigger, if it be cold or below a red heat.

To try this property in a different and more convincing way, I heated a large iron nail till it was white hot, and in that state placed it upon an earthen support near one pole of the magnetic needle, so as to lie not in the same direction, but on one side of it. Then, looking attentively on the graduated circle of the compass, I observed, that the needle was not in the least moved from its natural situation, whilst the nail remained red-hot ; but, as soon as the redness began to disappear, the needle advanced towards the nail, and a few seconds after the needle pointed directly towards it.

I tried whether in this experiment any difference was occasioned by the magnets being natural or artificial ; but, as it might be expected, there was none.

In pursuance of those magnetic experiments wherein heat is concerned, I tried the effects which took place when the magnet was heated ; but as the diminution of its power by heating, and an increase of it by cooling, were observed and described by the late Mr. CANTON, I shall only add a circumstance, which may perhaps be new. It is that an artificial magnet, after having had its power diminished by heating, does not recover it intirely again by cooling ; having constantly found, that the magnets which had been heated, after cooling would never hold as great a weight of iron as they did before. The heat to which those magnets were exposed never exceeded that of boiling water. This was rendered more evident by the following experiment.

A magnetic bar was placed in an earthen vessel at some distance from the south pole of the needle of a very good compass ; by the action of which magnet that end of the needle was drawn several degrees from the magnetic meridian, or the direction in which it stood before. In this situation of the
apparatus

apparatus boiling water was poured into the vessel wherein the magnet stood, in consequence of which the needle went back two degrees and a half. Some time after, when the water was quite cold, the needle was found nearer to the magnet, but not so near as it stood before the hot water was poured into the vessel.

Next to the effects of heat, I was desirous of trying what could be effected by decomposing the iron; and for this purpose an earthen vessel, containing about two ounces of iron filings, was placed near the south end of the needle of the compass, by which the needle was drawn a little out of its natural direction. Having marked where the needle now stood, some water first, and then some vitriolic acid, were poured upon the filings, which occasioned a brisk effervescence, and a copious production of inflammable air; but soon after the beginning of the effervescence, I was surprised to observe, that the needle came nearer to the vessel, shewing that the attraction between the needle and the filings had been increased by the action of the vitriolic acid upon the latter, which is contrary to what could have been expected; for if we consider that the power of a magnet is diminished by heat, and that red-hot iron has either no attraction at all, or an exceedingly small degree of it, towards the magnet, we might have concluded, that the action of the vitriolic acid upon iron would immediately diminish its attraction, besides the other strong reason arising from the dephlogistication of the iron occasioned by the effervescence; and in fact some time after, when the violence of the effervescence, and of course the production of inflammable air, begins to abate, the attraction between the needle and the filings begins likewise to diminish; and at last, when the effervescence is hardly perceptible, the needle is found to stand

stand farther from the vessel containing the filings, &c. than it stood before the vitriolic acid was added, which diminution of attraction is certainly owing to the loss of phlogiston; it being well known, that iron is less and less attracted by the magnet in proportion as it approaches nearer to the calcined state. Here follows the particular account of the above-mentioned experiment.

The south end of the needle coincided with the $285^{\circ} 15'$ on the divided circle. When the pot with the iron filings was placed on one side of it, the needle pointed to $286^{\circ} 15'$, being drawn just one degree nearer. Having added the diluted vitriolic acid to the filings, the needle came nearer, and stood at $286^{\circ} 45'$. Ten minutes after the beginning of the effervescence it stood at $286^{\circ} 35'$, having receded a little; and a few minutes after this observation it stood at $286^{\circ} 30'$. An additional quantity of diluted vitriolic acid was now added, which increased the effervescence considerably; and on observing it a short time after, the needle was found at the same point at which it stood before, from which time it began to go back very gradually; so that about three hours after it stood at $285^{\circ} 50'$, viz. farther from the effervescing mixture than it stood before any vitriolic acid was poured upon the iron filings.

As a single experiment ought not to be depended upon when an error may be occasioned by many concurring circumstances, I repeated this experiment with great precaution, taking care that nothing could shake the needle, or the rest of the apparatus; but the result was nearly the same, the attraction between the iron filings and the needle being increased by the action of the vitriolic acid.

In order to ascertain that this effect was not owing to the heat generated by the effervescence, the pot, with some iron

filings, was placed near the magnetic needle, as before : then some boiling water was poured upon the filings, which heated them much more than the diluted vitriolic acid could have done ; but the magnetic needle was not moved in the least from its original situation.

The suspicion which occurred next was, whether the effervescence might not agitate the iron filings so as to bring a greater number of them to that side of the vessel, which stands contiguous to the magnetic needle. In order to obviate this objection, the experiment was repeated with a single piece of iron instead of the filings ; but as this piece of iron presented a very small surface to the diluted acid, the effervescence was very weak, and of course the magnetic needle did not move when the acid was poured into the pot. At last, in order to remedy this second inconvenience, arising from the want of surface, I used a long piece of small steel wire, which was twisted in various directions, so as to be admitted into the pot ; in which case the metal presented a large surface to the acid, and it was not subject to be moved by the effervescence. The result was similar to that of the first experiment, *viz.* the attraction was increased by the action of the acid on the wire ; and here follows the particular account of the experiment.

About six yards of clean steel wire, somewhat less than one-fiftieth of an inch in diameter, being twisted in various directions, was put into an earthen vessel, which was placed near the south end of the magnetic needle, which in consequence of that was drawn from its natural situation, *viz.* from 281° to 280° . After adding the diluted vitriolic acid, a strong effervescence commenced, and the needle came to $279^{\circ} 47'$. About five minutes after, it stood at $279^{\circ} 35'$. Five minutes after this, it stood at $279^{\circ} 30'$. And a little after this observation, it ap-
peared

peared to be even somewhat nearer to the pot than the above-mentioned point. The experiment was then discontinued, and on removing the pot, the needle went back to its original situation, *viz.* 281° ; which shewed, that its alteration during the process was occasioned by the action of the acid on the iron, and not by any extraneous cause.

On examination the wire was found only blackened on its surface, but not nearly consumed; I had therefore the curiosity of trying the same wire again, and accordingly it was placed in the same vessel near the magnetic needle, which attracted the latter from its original situation 281° to 280° . After adding the acid, the needle came nearer, as in the preceding experiment; and a short time after it stood at $279^{\circ} 30'$, at which time the pot was removed, there being no occasion to continue the experiment any longer.

On pouring the liquor out of the pot, the wire did not appear to be much wasted. The pot was then replaced near the needle, so as to attract it a little nearer as before; but on pouring boiling water upon the wire, a pretty brisk effervescence took place, and the needle was in consequence attracted still nearer. This experiment shewed, that though the diluted acid had been poured out, yet there remained a quantity of it adhering to the wire, which was sufficient to renew the effervescence, when assisted by the heat of the boiling water.

Upon the whole, it appears, that the action of vitriolic acid upon iron or steel increases their attraction towards the magnet; that this increase of attraction has a limit, after which it begins to decrease; and that this limit seems to come sooner when iron than when steel is used; but, however, in respect to this last particular I am not yet quite certain, since, in the

experiments hitherto made, the variety in the shape or bulk of the iron or steel may have occasioned a considerable difference.

After the result of those experiments, it was natural to examine the effect which other acids might have on iron and steel; therefore the above-mentioned experiment of the steel wire was repeated with nitrous instead of vitriolic acid; the result of which was that the attraction between the magnetic needle and the wire was increased, but not so much as when vitriolic acid had been used. Here follows the particular account of the experiment.

About six yards of clean steel wire, near one-fiftieth of an inch in diameter, being twisted in various directions, was placed in the usual vessel near the south end of the needle; in consequence of which the needle was attracted from its natural situation, *viz.* from $283^{\circ} 32'$ to $282^{\circ} 50'$. About two ounces of water were then poured over the wire, and immediately after, near one ounce of nitrous acid was added, which produced an effervescence: the magnetic needle, however, hardly moved from its former situation; but in about a minute's time, the effervescence being increased very much, the needle came to $282^{\circ} 42'$. About a quarter of an hour after, the violence of the effervescence abated a little, and the needle went back again to $282^{\circ} 50'$. A short time after, it stood at $283^{\circ} 2'$. At last, when about half an hour had elapsed since the beginning of the operation, the effervescence was hardly perceivable, the liquor was become red like the colour of red ochre, and the needle stood at $283^{\circ} 15'$, *viz.* farther from the vessel than it did before the acid was added to the wire.

It appears, therefore, that the effervescence occasioned by the nitrous acid produced a similar effect, though not in so great a degree as the vitriolic. The maximum of the attraction seems to

to come sooner when nitrous than when vitriolic acid is used, after which limit the attraction decreases much faster in the former than in the latter case, which is evidently owing to the metal being more easily dephlogisticated and dissolved by the nitrous than by the vitriolic acid.

The marine acid was tried next; but, notwithstanding all the precautions I could take, it always occasioned a very weak effervescence, and the needle was not in the least affected by it.

A strong effervescence seems to be necessary to increase the attraction between the magnet and iron or steel; for when I tried the experiment by putting a small quantity of acid into the vessel, the effervescence was exceedingly weak; the magnetic needle was not at first affected by it, but several hours after it stood a little farther from the pot, which was evidently owing to the dephlogistication of the iron.

After observing the action of acids on iron, I next tried to decompose that metal by means of fire, to observe what effect would take place with respect to its magnetism. For this purpose two ounces of iron filings were mixed with an ounce of flowers of sulphur and an ounce of nitre. This mixture was put into a small and shallow earthen vessel, and was placed near the south extremity of the magnetic needle, which attracted the needle nearer than its natural situation by about one degree and a half. A pane of glass was interposed between the magnetic needle and the vessel. Things being thus disposed, the mixture was fired, and it burned rather slowly, the fire sometimes going out, so as to require being fired again; till at last it would burn no longer. During this combustion the needle was once attracted somewhat nearer; but its motion that way was so little, that I could not be quite certain of it. This happened not long after the fire was first communicated to the mixture,
after

after which the needle generally vibrated backwards and forwards, but upon the whole it gradually receded.

On repeating the experiment with a larger quantity of the mixture, and also with different proportions of ingredients, I could not observe any particular attraction. The needle vibrated rather more than before, but gradually receded; so that at last it stood farther from the vessel than it did before the mixture was set on fire, though not quite in its natural direction.

The vibration of the needle in this experiment, or its waving motion, was probably owing to the irregular burning of the mixture, and perhaps to the heating of one part of it whilst the other was burning. The gradual receding of the needle was certainly owing to the dephlogistication of the iron.

After having thus related the result of experiments, I shall now beg leave to add a few thoughts concerning the application of those observations towards accounting for the variation of the magnetic needle.

This wonderful phenomenon has, since it was first discovered, employed the thoughts of very able philosophers; many hypotheses having been offered, not only for its explanation, but even to foretel the future variations in various parts of the world. I need not detain this learned Society with a particular history of those hypotheses, but shall only observe in general, that neither their predictions have answered, nor any of them was founded upon evident principles. The supposition of a large magnet being inclosed within the body of the earth, and of its relatively moving with respect to the outward shell or crust; the supposition of there being four moveable magnetic poles within the earth; the hypothesis of a magnetic power, partly within and partly without the surface of the earth; together with several other hypotheses on the same subject, are not
only

only unwarranted by actual experiments, but do neither seem analogous to the other operations of nature. The late ingenious Mr. CANTON, Member of this Society, was the first, who endeavoured to account for the daily variation of the magnetic needle by the heating and cooling of the magnetic bodies in different parts of the earth's surface; which was in consequence of his having first observed, that the action of the magnet on the needle was diminished by heating, and increased by cooling*.

Following Mr. CANTON's judicious method of deriving the explanation of natural appearances from properties actually proved by experiments, I think, that the increase and diminution of magnetic attraction by heating and cooling of the magnet, as observed by Mr. CANTON, together with the result of the experiments recited in this Paper, seem fully sufficient to account for the general variation of the needle.

If we collect under one point of view all the causes hitherto ascertained, which can increase or diminish the attraction between magnetic bodies, we shall find, that the attraction between the magnet and iron, or between magnet and magnet, is increased by cooling, by a regeneration of iron or phlogistication of its calx, and within certain limitations by the action of acids upon the iron; that this attraction is diminished by heating, and by the decomposition of iron; and, lastly, that it is probably annihilated by a very great degree of heat.

These truths being premised, it must be considered, first, that, according to innumerable observations and daily experience, the body of the earth contains almost every where ferruginous bodies in various states and bulks; secondly, that the magnetic needle must be attracted by all those bodies, and its

* Philosophical Transactions, vol. LI. p. 398.

situation

situation or direction must be determined by all those attractions considered together, *viz.* from their common center of action; thirdly, that by removing or altering the degrees of attraction of some of those bodies which are situate on one side of the magnetic meridian, more than of those situated on the other side, the above-mentioned common center of attractions, and, of course, the direction of the magnetic needle must be altered, which in fact is the variation of the needle; and, lastly, that this alteration in the attractions of some of the ferruginous bodies in the earth must undoubtedly take place, it being occasioned by the parts of the earth being irregularly heated and cooled, by the action of volcanoes which decompose or otherwise alter large masses of ferruginous substance, by earthquakes which remove ferruginous bodies from their original places, and we may add also by the *aurora borealis*; for though we are as yet ignorant of the cause of that surprising phenomenon, it is however certain, that the magnetic needle has been frequently disturbed when the *aurora borealis* appeared very strong.

The magnetic needle, therefore, being necessarily affected by those causes, it seems unnecessary to have recourse to other hypothetical causes which are not established on actual experience.

In order to exemplify this explanation of the variation in a familiar manner, I made the following experiment, with the account of which I shall conclude this Paper. Four earthen vessels were disposed round the magnetic needle, two near its south, and the other two near its north pole, but not at equal distances. In one of those vessels there was placed a natural magnet; the second contained several small bits of magnetic steel mixed with earth; and in each of the other two there were
put

put about four ounces of iron filings. Things being thus disposed, and left undisturbed for about half an hour, the needle remained unaltered. Then the pieces of magnetic steel and earth were stirred with a stick, in consequence of which the needle was agitated. After this, some diluted vitriolic acid was poured upon the filings in one of the vessels, the action of which attracted the needle that way; but whilst the needle remained in that situation, some diluted vitriolic acid was poured upon the iron filings in the other vessel, which stood on the other side, in consequence of which the needle went back again towards its former direction. Whilst the effervescences were going on in the two vessels, the magnet in the first vessel was heated by means of boiling water, which occasioned another alteration in the direction of the magnetic needle; and thus, by altering the state of the ferruginous substances in the vessels, the needle's direction was altered, in evident imitation of the natural variation.

T. CAVALLO.



IV. *Description of a new Electrometer. In a Letter from the Rev. Abraham Bennet, M. A. to the Rev. Joseph Priestley, LL.D. F. R. S.*

Read December 7, 1786.

R E V. S I R,

Wirksworth, Sept. 14, 1786.

I SEND you a description of my electrometer, which, having the honour of your approbation, may be communicated to the Royal Society. (See Tab. II. fig. 1. and 2. which represent two sections of the instrument.)

It consists of two slips of leaf gold, *aa*, suspended in a glass *b*. The foot *c* may be made of wood or metal; the cap *d* of metal. The cap is made flat on the top, that plates, books, evaporating water, or other things to be electrified, may be conveniently placed upon it. The cap is about an inch wider in diameter than the glass, and its rim about three-quarters of an inch broad, which hangs parallel to the glass, to turn off the rain and keep it sufficiently insulated. Within this is another circular rim, about half as broad as the other, which is lined with silk or velvet, and fits close upon the outside of the glass; thus the cap fits well, and may be easily taken off to repair any accident happening to the leaf gold. Within this rim is a tin tube *e*, hanging from the center of the cap, somewhat longer than the depth of the inner rim. In the tube a small peg *f* is placed, and may be occasionally taken out. To the
peg,

peg, which is made round at one end and flat at the other, two slips of leaf gold are fastened with paste, gum-water, or varnish. These slips, suspended by the peg, and that in the tube fast to the center of the cap, hang in the middle of the glass, about three inches long, and a quarter of an inch broad. In one side of the cap there is a small tube *g*, to place wires in. It is evident, that without the glass the leaf gold would be so agitated by the least motion of the air, that it would be useless; and if the electricity should be communicated to the surface of the glass, it would interfere with the repulsion of the leaf gold; therefore two long pieces *bb* of tin-foil are fastened with varnish on opposite sides of the *internal* surface of the glass, where the leaf gold may be expected to strike, and in connexion with the foot. The upper end of the glass is covered and lined with sealing-wax as low as the outermost rim, to make its insulation more perfect. Tab. III. fig. 1. represents the instrument joined together, and ready for use.

The following experiments will shew the sensibility of this instrument. See Tab. IV.

1st, Powdered chalk was put into a pair of bellows, and blown upon the cap, which electrified it positively when the cap was about the distance of six inches from the nozzle of the bellows; but the same stream of powdered chalk electrified it negatively at the distance of three feet, as represented in fig. 2. and 3. In this experiment there is a change of electricity from positive to negative, by the dispersion or wider diffusion of the powder in the air. It is also changed by placing a bunch of fine wire, silk, or feathers, in the nozzle of the bellows, and is wholly negative when blown from a pair of bellows without their iron pipe, so as to come out in a larger stream; this last experiment did not answer in dry weather so well as in wet.

The positive electricity of the chalk, thus blown, is communicated because part of the powder sticks to the cap; but the negative is not communicated, the leaf gold collapsing as soon as the cloud of chalk is dispersed.

2dly, A piece of chalk drawn over a brush, or powdered chalk put into the brush, and projected upon the cap, electrifies it negatively; but its electricity is not communicated. Fig. 4.

3dly, Powdered chalk blown with the mouth or bellows from a metal plate placed upon the cap, electrifies it permanently positive. Fig. 5. Or if the chalk is blown from the plate, either insulated or not, so that the powder may pass over the cap, if not too far off, it is also positive. Or if a brush is placed upon the cap, and a piece of chalk drawn over it, when the hand is withdrawn the leaf gold gradually opens with positive electricity as the cloud of chalk disperses.

4thly, Powdered chalk falling, from one plate, to another placed upon the instrument, electrifies it negatively. Fig. 6.

Other methods of producing electricity with chalk and other powders have been tried; as projecting chalk from a goose wing, chalking the edges of books and clapping the book suddenly together, also sifting the powder upon the cap; all which electrified it negatively: but the instrument being placed in a dusty road, and the dust struck up with a stick near it, electrified it positively. Breaking the glass-tear upon a book electrified it negatively, probably by friction in the act of shivering, for when broken in water it did not electrify it.

Wheat flour, and red lead, are strongly negative in all cases where the chalk is positive. The following powders were like chalk: red ochre and yellow, rosin, coal ashes, powdered crocus metallorum, aurum mosaicum, black-lead, lampblack (which was only sensible in the two first methods), powdered quick-

quick-lime, umber, lapis calaminaris, Spanish brown, powdered sulphur, flowers of sulphur, iron filings, rust of iron, sand. Rosin and chalk, separately alike, were changed by mixture; this was often tried in dry weather, but did not succeed in damp: white lead also sometimes produced positive, and sometimes negative, when blown from a plate.

If a metal cup be placed upon the cap, with a red-hot coal in it, a spoonful of water thrown in electrifies the cup negatively; and if a bent wire be placed in the cap, with a piece of paper fastened to it to increase its surface, the positive electricity of the ascending vapour may be tried by introducing the paper into it. Perhaps the electrification of fogs and rain is well illustrated by pouring water through an insulated cullender, containing hot coals, where the ascending vapour is positive, and falling drops negative. Fig. 7.

The sensibility of this electrometer may be considerably increased by placing a candle upon the cap. By this means a cloud of chalk, which only just opens the leaf gold, will cause it to strike the sides for a long time together; and the electricity, which was not before communicated, now passes into the electrometer, causing the leaf gold to repel, after it is carried away. Even sealing-wax by this means communicates its fire at the distance of twelve inches at least, which it would scarcely otherwise do by rubbing upon the cap.

A cloud of chalk or wheat flour may be made in one room, and the electrometer, with its candle, be afterwards leisurely brought from another room, and the cloud will electrify it before it comes very near. The air of a room, adjoining to that wherein the electrical machine was used, was very sensibly electrified, which was perceived by carrying the instrument through it with its candle. Fig. 8.

In

In very clear weather, when no clouds were visible, the electrometer has been often applied to the insulated string of kites without metal, and their positive electricity caused the leaf gold to strike the sides; but when a kite was raised in cloudy weather, with a wire in the string, and when it gave sparks about a quarter of an inch long, the electricity was sensible by the electrometer at the distance of ten yards or more from the string; but, when placed at the distance of six feet, the leaf gold continued to strike the sides of the electrometer, for more than an hour together, with a velocity increasing and decreasing with the density or distance of the unequal clouds which passed over.

Sometimes the electricity of an approaching cloud has been sensible without a kite, though in a very unfavourable situation, for it, being in a town surrounded with hills, and where buildings encompassed the wall on which the electrometer was placed. A thunder cloud passing over, caused the leaf gold to strike the sides of the glass very quick at each flash of lightning.

No sensible electricity is produced by blowing pure air, projecting water, by smoke, flame, or explosions of gunpowder.

A book was placed upon the cap, and struck with silk, linen, woollen, cotton, parchment, and paper, all which produced negative repulsion; but when the other side of the book was struck with silk, it became positive; this side, struck at right angles with the former, was again negative; and, by continuing the strokes which produced positive, it changed to negative, for a little while; and, by stopping again, became positive. No other book would do the same, though the sides were scraped unchalked, upon a supposition that altering the surface would produce it. At last, one side of a book was moistened, which changed it; so I concluded, that one edge

of the book had lain in a damp place; which conjecture was farther confirmed by all the books becoming positive in damp weather, and one of them being dried at the fire again became negative.

When the cap is approached with excited sealing-wax, the leaf gold may be made to strike the sides of the glass more than twelve times; and as the sealing wax recedes, it strikes nearly as often; but, if it approaches much quicker than it recedes, the second number will sometimes be greater.

The quantity of electricity necessary to cause a repulsion of the leaf gold is so small, that the sharpest point or edges do not draw it off without touching; hence it is unnecessary to avoid points or edges in the construction of this instrument.

I am, &c.

ABRAHAM BENNET.



V. Appendix to the Description of a new Electrometer. In a Letter from the Rev. Abraham Bennet, M. A. to Charles Blagden, M. D. Sec. R. S.

Read December 21, 1786.

S I R,

Wirksworth, Dec. 18, 1786.

THE following description of my Electrometer, connected with M. VOLTA's Condenser, waits on the approbation of the Royal Society, to be inserted in the Philosophical Transactions, as an Appendix to my Paper, communicated by Dr. PRIESTLEY.

The metallic cap of the electrometer is for this purpose to be ground and polished flat and smooth, to fit a piece of marble also polished on both sides, and varnished. In the side of the marble is to be fixed a glass or baked-wood handle. Lastly, on the top is a smaller metallic plate, furnished also with an insulating handle. The whole construction will be understood by the annexed drawing (see Table III. fig. 2.). When a small charge of electricity is communicated to the metal at A, whilst the marble B is touched, the single condenser is charged, and its electricity (if in sufficient quantity) will be sensible when the marble is lifted up by the handle C; but, if not now sensible, touch the small plate D, whilst thus lifted up from the cap, and then lift it off the marble by its insulating handle, and pre-
2
senting

senting it to the cap of the electrometer (if not still in too small a quantity) it will cause a divergence of the leaf gold, by an electricity of the same kind with that which was communicated to the cap, and in which cap a small quantity of electricity remains. Thus both the larger and smaller condensers of M. VOLTA are connected with the electrometer, so as to be used in the most simple, expeditious, and convenient manner I can think of. Their amazing power of condensing electricity is sufficiently explained in M. VOLTA's Paper, before published in the Philosophical Transactions.

To the experiments on blowing powders from a pair of bellows I have to add, that if the powder is blown at about the distance of three inches upon a plate which is moistened or oiled, its electricity is contrary to that produced by blowing upon a dry plate. This shews that the electricity of the streams of powder issuing out of the bellows is only contrary to the more expanded part, because it is within the influence of its atmosphere; for when this is destroyed by the adhesion of the powder to the moistened plate, it is negative when the bellows are positive, as it was before positive when the more expanded cloud was negative.

I have also to add, that the experiments on evaporation of water may be tried with more ease and certainty of success by heating the small end of a tobacco pipe, and pouring water into the head, which, running down to the heated part, is suddenly expanded, and will shew its electricity when projected upon the cap of the electrometer, more sensibly than any other way I have tried. If the pipe be fixed in a cloven stick, and placed in the cap of one electrometer, whilst the steam is projected upon another, it produces both electricities at once.

Spirit of wine and ether are electrified like water. Oil and vitriolic acid produced smoke without any change of electricity. In these experiments a long pipe is better than a short one.

I am, &c.

ABRAHAM BENNET.



Fig. 3.

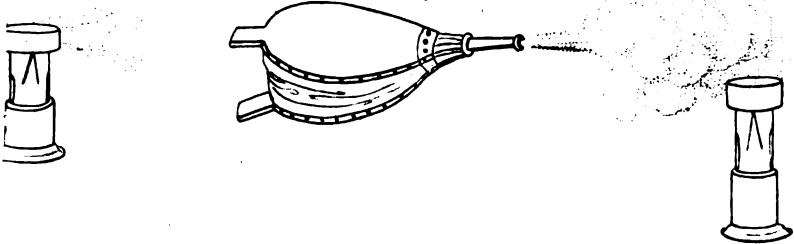


Fig. 5.

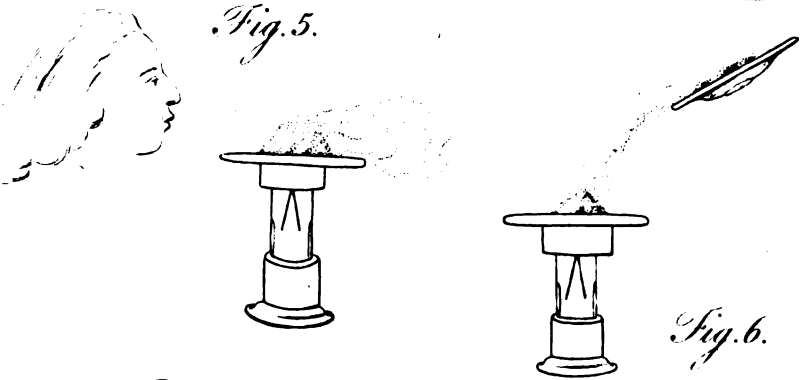
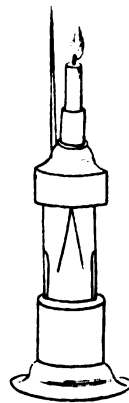


Fig. 6.



Fig. 8.



VI. *Some Account of an Earthquake felt in the Northern Part of England. In a Letter from Samuel More, Esq. to Sir Joseph Banks, Bart. P. R. S.*

Read December 7, 1786.

S I R,

Castle-Head, Lancashire, August 22, 1786.

I TAKE the liberty of addressing this to you, to give you information, and furnish you with all the particulars I have been able to collect, relative to a shock of an earthquake felt in this part of the kingdom on Friday, the 11th instant, about two o'clock in the morning; some say it was a little before that hour, others a little after, owing probably to the difference of the clocks.

I was at that time in the house of a miner at Aulstone-Moor in Cumberland, but perceived no shock; nor do I believe it was felt in that neighbourhood, as no mention was made of it by any of the miners I conversed with during the course of the day; but, on my arrival at Penrith in the evening, every one there spoke of it as having been sensibly felt in that town. The next day, pursuing my journey, I was informed it had been felt along the banks of Ulswater, in Patterdale, at Ambleside, along the side of Winander Meer, and particularly at the house in the island on that lake, the property of Mr. CHRISTIAN. At the place where I am now writing, the Lady of the house, and some of the servants, were awakened by it, and describe it as

F 2

shaking

shaking violently the beds, the chairs in the rooms, and the sashes of the windows. At Cartmeal, a town about five miles from hence, it was also felt very severely; and at the village of Carke, two miles from Cartmeal, a gentleman (Mr. FLETCHER STOCKDALE) tells me, he was awake some time before the shock; that he first heard a rumbling noise, like a carriage at a distance, and was considering what carriage could be moving at that hour, when he felt the shock. The noise continued some time after the shock was over; and he thinks the whole might last about four or five seconds, and it seemed to travel from the east to the westward. Almost every body in the neighbourhood of Carke and Cartmeal were awakened by it, and some persons much alarmed; but I do not find that, at any part where I have been, any damage has been done by it. At Lancaster, about ten miles east of Cartmeal, it was very plainly felt, particularly, as I am told, in the great tower of the Castle. It appears to have extended as far as Manchester, where it was slightly perceived.

These are the particulars I have hitherto been able to collect relative to this earthquake, of which I doubt not you will have many accounts sent you; but if these facts furnish any thing not mentioned by your other correspondents, it will afford great pleasure to, Sir, &c.

SAMUEL MORE.



VII. *Determination of the Heliocentric Longitude of the descending Node of Saturn. By Thomas Bugge, Professor of Astronomy in the University of Copenhagen; communicated by Sir Joseph Banks, Bart. P. R. S.*

Read December 7, 1786.

THE culmination of Saturn was observed with a 6-feet achromatic transit-instrument, and the planet compared with σ and π of Sagittarius, whose apparent right-ascensions in the middle of August 1784 were $282^{\circ} 56' 54''$ and $284^{\circ} 14' 33''$. The meridian altitude was observed with a 6-feet mural quadrant. The original observations are to be published in the second volume of my Astronomical Observations. From those are calculated the right-ascension and declination, the geocentric longitude and latitude, of Saturn, which are to be depended upon to 4 or 6 seconds. Those observed longitudes and latitudes are compared with the tables of Dr. HALLEY and of M. DE LA LANDE. In the errors of the tables + signifies that the longitude of the tables is less than the observed longitude; and the meaning of - is, that the calculated longitude is greater than the observed. It ought to be observed, that the heliocentric longitudes of Dr. HALLEY's Tables have been corrected for the perturbations after the principles of M. LAMBERT (Memoires de Berlin, pour 1783, p. 216. and Collection des Tables Astronomiques de Berlin, tom. II. p. 269.)

1784	$\frac{1}{2}$ culmi- nation, mean time at Copen.			$\frac{1}{2}$ observed longitude.			$\frac{1}{2}$ observ- ed lati- tude.			The error of HALLEY		The error of M. DE LA LANDE.	
										in long.	in lat.	in long.	in lat.
	h.	'	"	h.	'	"	h.	'	"				
July 12	12	3	19	20	34	48	0	3	35 B	+ 2 22	+ 32	- 9 40	+ 33
20	11	29	99	19	59	39	0	2	59	+ 2 14	+ 38		
Aug. 1	10	38	25	9	19	9	22	0	1 44	+ 1 45	+ 27	- 9 49	+ 28
8	10	9	09	18	42	56	0	1	2	+ 1 48	+ 21		
21	9	14	59	9	18	1	23	0	2	+ 1 35	+ 28		
27	8	50	19	9	17	46	19	0	29 A	+ 1 26	- 26	- 9 35	- 30
31	8	33	47	9	17	38	7	0	53	+ 1 19	- 23		
Sept. 5	8	13	45	9	17	29	36	0	1 26	+ 1 20	- 25	- 9 25	- 27
15	7	33	45	9	17	19	39	0	2 4	+ 1 16	- 25		
Oct. 8	6	4	23	9	17	34	6	0	3 57	+ 1 22	- 26	- 8 48	- 32

In order to reduce the observed geocentric longitude to the sun, or by observation to find the heliocentric longitude, it is required to know the angle at the planet = p . If this angle is calculated in the common way only by the tables, there will arise some difference, according to the different elements and the different constructions of the tables. Thus, at the time of Saturn's culmination, this angle is found the 12th of July, by the tables of Dr. HALLEY = $0^{\circ} 3' 13''$, and by the tables of M. DE LA LANDE = $0^{\circ} 2' 0''$; the 8th of August by HALLEY = $2^{\circ} 43' 35''$, and by M. DE LA LANDE = $2^{\circ} 42' 34''$; the 27th of August after Dr. HALLEY = $4^{\circ} 14' 15''$, and after M. DE LA LANDE = $4^{\circ} 13' 47''$. To avoid those differences, which often may alter the heliocentric longitude more than one or two minutes, the following method may be useful. The heliocentric longitude of the earth, calculated after the tables of M. MAYER, is to be depended upon to eight or ten seconds. From the heliocentric longitude of the earth, and from the observed geocentric longitude of the planet, corrected for the aberration and nutation, is deduced the angle at the earth = γ ,

or the distance between the sun and the planet seen from the earth. The dimensions of the elliptical orbit of the planet are so far ascertained, that the logarithms of the distance from the sun have not any material difference in the different tables. From the angle t , the distance of the earth from the sun, and the distance of the planet from the sun, the angle p is calculated to a sufficient degree of accuracy. Thus, the 12th of July, by the distances of Dr. HALLEY, $p = 0^{\circ} 2' 59''$, and by the distances of M. DE LA LANDE $p = 0^{\circ} 2' 59''$; the 8th of August after Dr. HALLEY $p = 2^{\circ} 43' 25''$, and after M. DE LA LANDE $p = 2^{\circ} 43' 36''$; the 27th of August after Dr. HALLEY $p = 4^{\circ} 14' 10''$, and after M. DE LA LANDE $p = 4^{\circ} 14' 28''$. The difference very seldom will amount to 20 seconds, and is of no consequence in this matter. From the observed geocentric latitude of the angle at the sun $= s$, and the angle at the earth $= t$, the heliocentric latitude of the planet is found $=$

$$\frac{\text{tang. lat. geoc.} \times \sin. s}{\sin. t}$$

1784.	Mean time at Copenhagen.	h observed helio- centric longitude.	h observed helio- centric latitude.
	h. ' "	s. ' "	° ' " B
July 12	12 3 1	9 20 37 29	0 3 13 B
20	11 29 9	9 20 51 53	0 2 41
Aug. 1	10 38 85	9 21 13 17	0 1 34
8.	10 9 0	9 21 26 2	0 0 56
21	9 14 59	9 21 49 27	0 0 2
27	8 50 19	9 22 0 12	0 0 27 A
31	8 33 47	9 22 7 32	0 0 50
Sept. 5	8 13 45	9 22 16 28	0 1 21
15	7 33 45	9 22 34 32	0 1 59
Oct. 8	6 4 23	9 23 16 15	0 3 35



When two heliocentric longitudes, and the corresponding northern and southern latitude are given, the distance of

of the node from one of the longitudes or places may be found. Let DE be the ecliptic, AF the orbit of the planet, N the node, DE the difference between the two observed heliocentric longitudes = a , EF the southern latitude = β , AD the northern latitude = b , NE the distance of the node from the heliocentric place at E, and corresponding to the southern latitude = x . In the spherical triangles ADN and FEN,

$$\frac{\sin. (a-x)}{\text{tang. } b} = \cot. N = \frac{\sin. x}{\text{tang. } \beta}.$$

By placing the value of $\sin. (a-x)$ in the equation

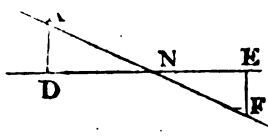
$$\frac{\sin. a \cdot \cos. x - \sin. x \cdot \cos. a}{\text{tang. } b} = \frac{\sin. x}{\text{tang. } \beta}.$$

By resolving this equation

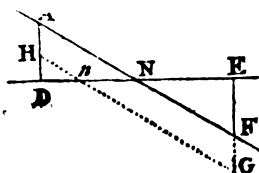
$$\frac{\sin. x}{\cos. x} = \text{tang. } x = \frac{\sin. a \cdot \text{tang. } \beta}{\text{tang. } b + \cos. a \cdot \text{tang. } \beta}.$$

If a , b , and β , are very small arcs, which commonly is the case with the planets, then $\sin. a = a$, $\text{tang. } \beta = \beta$, $\text{tang. } b = b$, and $\cos. a = 1$. Hence the spherical formula will be transformed into another $x = \frac{a\beta}{b+\beta}$. This formula belongs to

plane geometry, and may besides be thus demonstrated. DN:NE = AD:EF. Hence DN + NE : NE = AD + EF : EF; and NE = $\frac{DE \times EF}{AD + EF}$. If



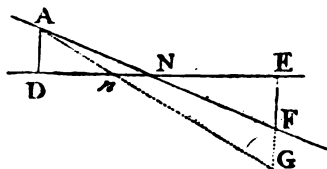
the difference of the longitudes do not exceed one degree, and the latitudes are not greater than ten minutes, the spherical and the rectilineal formula will agree to very few seconds. Small faults in the longitude will not very much alter the true place of the node; but very small errors in the latitude are of great consequence. Let the error in the



southern heliocentric latitude be $FG = +d$. The error in the northern latitude $AH = -d$. Hence $DH : Dn = GE : En$, and $En = \frac{a(\beta + d)}{b + \beta}$. By subtracting EN

$$= \frac{a\beta}{b+\beta}, \text{ the error in the heliocentric longitude of the node,}$$

$Nn = \frac{ad}{b+\beta}$. If the fault in the southern latitude = $-d$, in the northern latitude = $+d$, the same formula is still true; but then $EN > En$, and the place of the erroneous node will be between E and N. In both cases the errors in the place of the node are directly as the errors in the latitudes.



Let us now suppose, that only the one latitude is erroneous $\pm d$. Then

$$Nn = \pm En \mp EN \mp a \times \left(\pm \frac{\beta \pm d}{b + \beta \pm d} \mp \frac{\beta}{b + \beta} \right) = \frac{abd}{(b + \beta)^2 \pm d(b + \beta)}. \quad \text{In the case}$$

when the error in both latitudes is positive $= +d$, and $\beta > b$, or $\beta < b$, the resulting error in the place of the node $=$

$$\frac{ad(\mp b \pm \beta)}{(b + \beta)^2 + 2d(b + \beta)}.$$
 In the case when the error in both latitudes is

negative $-d$, and $\beta > b$, or $\beta < b$, then the error in the node =

$$\frac{ad(\mp b \pm \beta)}{(b + \beta)^2 - 2d(b + \beta)}.$$
 In those two cases the error is less than in

any of the former, and quite nothing when $b=\beta$. If the radius of the instrument, with which the meridian altitudes are observed, is given, the quantity of d is also given. In a mural quadrant of 6 or 8 feet $d=5$ or 3 seconds. Take $a=34' 3''$, $b=56''$, $\beta=27''$, $d=5''$, and the error in the southern latitude $+d$, in the northern $\doteq -d$; then $Nn \doteq \frac{10215''}{83} \doteq$

2' 12''. Take now the error only in the southern latitude
 $= +d$; then $Nn = \frac{572040''}{7304} = 1' 18''$; in the case of $-d$; $Nn =$

$\frac{572040''}{6474} = 1' 28''$. From hence it appears, that in comparing

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two

two single observations, it scarce will be possible to avoid a fault of ± 2 minutes in the place of the node.

If the instrument is of a less force than a mural quadrant of 6 feet, and the possible faults in the altitudes greater, for example, 10 or 15 seconds, the resulting error in the place of the node may very easily be calculated; but the error in the node will be enormous, and the observations of no use for a nice astronomer.

Compared Observations 1784.	λ heliocentric longitude on the last day.	λ distance from the \odot .	Heliocentric longitude of $\lambda \odot$
	s. ° ' "	° ' "	s. ° ' "
July 12 with Sept. 15.	9 22 34 32	0 44 38	9 21 49 54
July 12 — Oct. 8	9 23 16 15	1 23 41	9 21 52 36
July 20 — Sept. 15	9 22 34 32	0 43 37	9 21 50 55
July 20 — Oct. 8	9 23 16 15	1 22 33	9 21 53 42
Aug. 1 — Sept. 5	9 22 16 28	0 29 15	9 21 47 13
Aug. 1 — Sept. 15	9 22 34 32	0 45 23	9 21 49 9
Aug. 1 — Oct. 8	9 23 16 15	1 25 34	9 21 50 41
Aug. 8 — Aug. 27	9 22 0 12	0 11 7	9 21 49 5
Aug. 8 — Aug. 31	9 22 7 32	0 19 34	9 21 47 58
Aug. 21 — Aug. 27	9 22 0 12	0 10 0	9 21 50 12
Aug. 21 — Aug. 31	9 22 7 32	0 17 23	9 21 50 9
Mean			9 21 50 8,5

This mean agrees pretty well with the observations on the 21st, 27th, and 31st of August, which are nearest the node, and most to be depended upon.

The 21st of August, at 9 h. 12' 26" true time at Copenhagen, the heliocentric longitude of Saturn = 9 s. 21° 49' 27", and the distance from the node = 41". The 27th of August, at 8 h. 49' 23", the heliocentric longitude = 9 s. 22° 0' 12"; therefore, in 5 days 23 h. 36' 57" Saturn has described an arc of 10' 45", and 10' 45" : 5 d. 23 h. 36' 57" = 41" : *. Hence

Saturn has spent 9 h. 7' 44" in going through those 41"; and *Saturn's passage through the node happened August 21, 1784, at 18 h. 20' 10", and the heliocentric longitude of his descending node = 9 s. 21° 50' 8",5.* The errors in the place of the node are relative to the tables of Dr. HALLEY + 19' 39", to the tables of M. CASINI + 16' 4", and to the tables of M. DE LA LANDE + 1' 31".

In the foregoing computation of Saturn's heliocentric longitude from the tables of Dr. HALLEY, this longitude has been corrected for the perturbation after the principles of M. LAMBERT. Though the geocentric places, calculated in this manner, will agree still better with the observations than without those perturbations, nevertheless they are only empiric, and not founded upon the theory and principles of gravitation; I shall therefore conclude this Paper, by adding the faults in the heliocentric places of Saturn, calculated only and directly from the tables of Dr. HALLEY, which may be of some use to improve those valuable tables,

1784	h heliocentric longitude from Dr. HALLEY's tables.	Error in longitude.	h heliocentric latitude from Dr. HALLEY's tables.	Error in latitude.
July 12	9 20 27 40	+ 9 49	0 2 45 B	+ 28
20	9 20 42 3	+ 9 50	0 2 17	+ 24
Aug. 1	9 21 3 41	+ 9 36	0 1 10	+ 24
8	9 21 16 19	+ 9 43	0 0 37	+ 21
21	9 21 39 45	+ 9 42	0 0 24 A	- 22
27	9 21 50 34	+ 9 38	0 0 53	- 26
31	9 21 57 47	+ 9 45	0 1 11	- 21
Sept. 5	9 22 6 48	+ 9 40	0 1 45	- 24
15	9 22 24 50	+ 9 42	0 2 22	- 23
Oct. 8	9 23 6 33	+ 9 44	0 4 1	- 20



VIII. *Description of a Set of Halo's and Parbelia, seen in the Year 1771, in North-America. By Alexander Baxter, Esq.; communicated by Sir Joseph Banks, Bart. P. R. S.*

Read December 7, 1786.

EXTRACT from a journal kept in the upper countries of North-America. At Fort Gloucester, on the river of Lake Superior, six miles above the falls of St. Mary's, and as much from the head of the river, where it issues from the Lake.

“ January 22, 1771. Last night and to-day the frost has been more severe than at any time this winter: I was hardly able, at mid-day, to keep my face to the wind uncovered, though the sun shone very bright, and the sky clear.

“ In the morning the wind was easterly, which went about with the sun to the south and westward, returning to the east in the evening; a very small breeze.

“ A little before two o'clock P.M. observed as follows. There was a very large circle or halo round the sun (see Tab. V.) within which the sky was thick and dusky, the rest of the hemisphere being clear; and, a little more than one-third way from the horizon to the zenith, was a beautifully enlightened circle, parallel to the horizon, which went quite round, till the two ends of it terminated in the circle that surrounded the sun; where

where, at the points of intersection, they each formed a luminous appearance about the bulk of the sun, and so like him when seen through a thick hazy sky, that they might very easily have been taken for him. (Mock suns or parhelia). Directly opposite to the sun was a luminous cross, in the shape of a St. Andrew's Cross, cutting at the point of intersection the horizontal circle, where was formed another mock sun, like the other two mentioned above. The two lower limbs of the cross appeared but faintly a little way below the circle, the two higher reached a good way above the circle towards the zenith very clear and bright. In this horizontal circle, directly half-way betwixt the sun of the cross and those at the ends of the same circle, were other two mock suns, same kind and size, one on each side; so that in this horizontal circle were five mock suns, at equal distances from one another, and in the same line the real sun, all at equal heights from the horizon. Besides these meteors, there was, very near the zenith, but a little more towards the circle of the real sun, a rainbow of very bright and beautiful colours, not an entire semi-circle, with the middle of the convex side turned towards the sun, which lowered as the sun descended.

“ It was a little before two o'clock P.M. when I first observed this phænomenon, and it continued in all its beauty and lustre till about half after two.

“ The cross went gradually off first; then the horizontal circle began to disappear in parts, while in others it was visible; then the three mock suns farthest from the sun, the two in the sun's circle continuing longest; the rainbow began to decrease after these; and, last of all, the sun's circle, but it was observable at three o'clock, or after it.

“ The

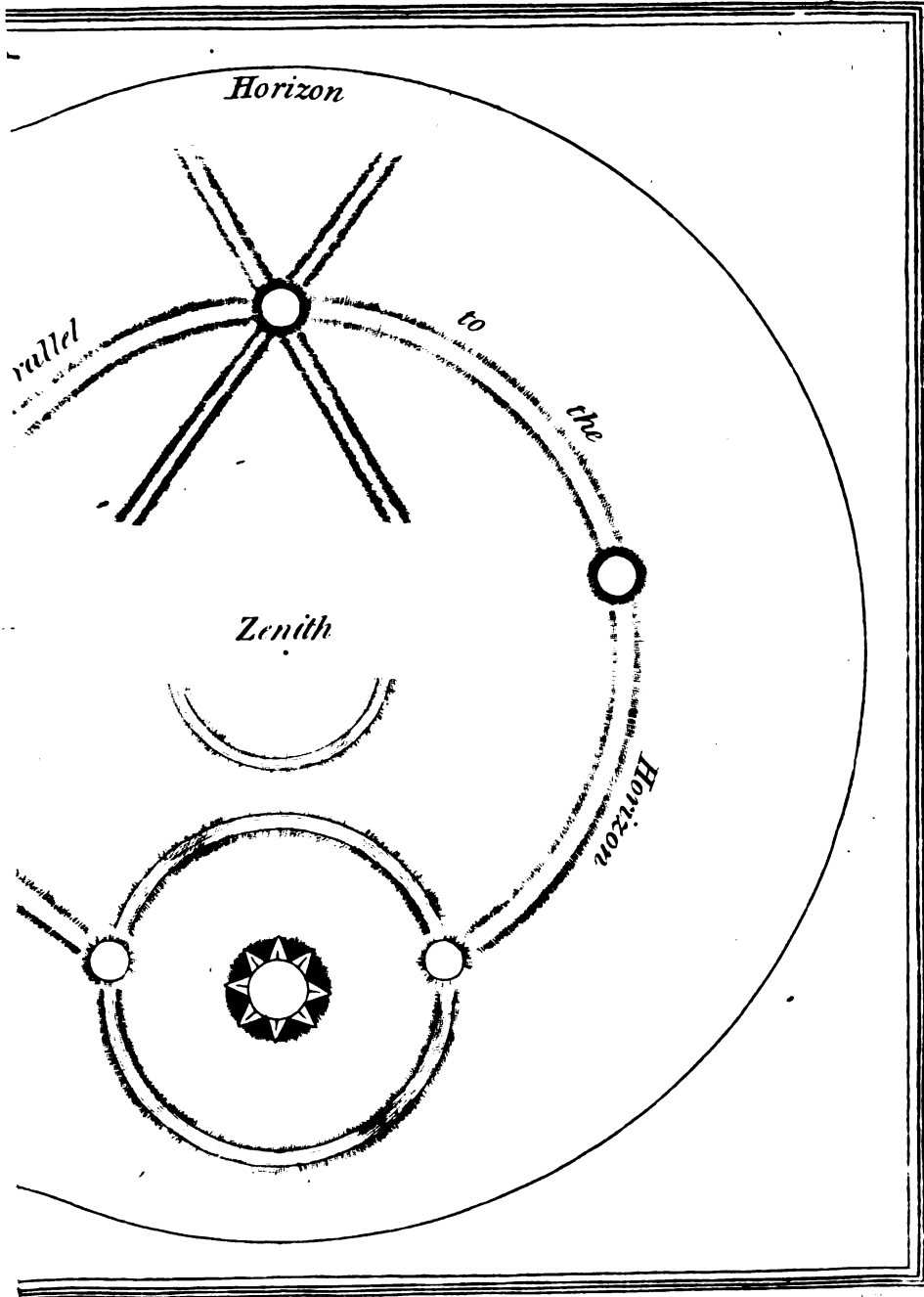
"The weather continued fine, but the next day was a little softer."

I have subjoined a diagram of the phenomenon, in order to give a better idea of it than my feeble description, which I have copied literally, as it was set down in my journal at the time, and in a hurry.

ALEX. BAXTER.

Odiham, August 21, 1785.





IX. Observations of the Transit of Mercury, May 4, 1786, at Dresden. By M. Köhler, Inspector of the Mathematical Repository of the Elector of Saxony; communicated by the Count de Brühl, F. R. S.

Read December 21, 1786.

Apparent time.

h.
9 21 54 beginning of the planet's egress, doubtful.
9 25 23 complete egress, or last contact, very certain.

The telescope with which Mr. KÖHLER observed, was a 9-foot refractor of DOLLOND's, magnifying 104 times.

He has made a comparison of his observation with that of ALEXANDER AUBERT, Esq. from which he has inferred the longitude of Dresden in the following manner.

	Mean time.
	h. ' "
Exterior contact observed by Mr. AUBERT at	8 27 5
Equation	+ 3 28,3
Difference of parallax	+ 0 13,4
Apparent time at Loam-pit Hill . . .	8 30 46,7
Loam-pit Hill west of Greenwich . .	+ 0 5,4
Apparent time at Greenwich	8 30 52,1
Apparent time at Dresden	9 25 23,0
Longitude of Dresden east of Greenwich .	54 30,9



*X. Observations of the Transit of Mercury at St. Petersburg.
In a Letter from M. Rumovki, Astronomer in the Imperial
Academy, to Mr. J. H. de Magellan, F. R. S.*

Read December 21, 1786.

J'AI l'honneur de vous communiquer mon observation sur le dernier passage du Mercure, 1786, le $\frac{22 \text{ Avril}}{3 \text{ Mai}}$, t. astr.

Le contact interne à l'entrée $17^{\text{h}} 2' 19''$ t.v. Limbes ondoyans.

Le contact interne à la sortie $22 26 55$. . . bien terminés.

M. TZERNOI, qui m'a accompagné dans l'observation, n'a vu que le contact interne à la sortie, $22^{\text{h}} 27' 7''$.

J'omets les contacts externes : pour le premier, j'ai manqué de le voir ; j'appercus le ☿ au ☉ à $16^{\text{h}} 59' 44''$, quand la moitié à-peu-près en a été entrée : et pour le contact externe à la sortie, j'ai douté jusqu'à la demi-minute.

Durant le passage, j'ai réussi à mesurer avec un micromètre objectif quelques distances des bords, et le diamètre de ☿ ; et je l'ai trouvé tantot de $7'',56$, et tantot de $8'',64$; en sorte que je l'estime de $8''2$, ou plutôt de $8''4$.

Depuis peu j'eus le loisir de calculer mes observations sur les distances : et en supposant le demi-diamètre du ☉ $15' 52''$, et la parallaxe $8'',5$, par 28 combinaisons je trouve :

La

La plus courte distance	11' 32"
Tems du milieu du passage	^{h.} 19 44' 37" pour le merid. de Peterß.
Tems de la conjonction	19 13 33 avec
La latitude géocentrique	11 43
La longitude géocentrique	1 13° 50' 1"
Et la longitude du Nœud	1 15 53 56

Pour reduire mon observation au centre, je trouve qu'il faut ôter du premier contact interne 1' 41'', et ajouter 1' 16'' au contact interne à la sortie: en sorte que suivant les élémens ci-dessus mentionnés, mon observation à l'entrée pèche en excès de 29'', et à la sortie de 4'' en défaut.

(Signed)

RUMOVSKI.



XI. *An Account of the Strata observed in sinking for Water at Boston, in Lincolnshire. By Mr. James Limbird, Surveyor to the Corporation; communicated by Sir Joseph Banks, Bart. P. R. S.*

Read December 21, 1786.

ON the 7th day of May, 1783, GEORGE NAYLOR, of Louth, in the county of Lincoln, Well-borer, began to bore at the well in the Market-Place, Boston; which had been sunk and bored to the depth of 186 feet from the surface, in 1747, by THOMAS PARTRIDGE.

The well was made about 6 feet in diameter at the top, 5 feet in diameter at the bottom, and 27 feet deep, and the earth prevented from falling in by a circular frame of wood, which goes from the surface of the earth to the depth of 21 feet and 6 inches, and is there supported by brick-work, laid on a bed of light-coloured blue clay, which continues to the depth of 36 feet from the surface, where there is a bed of sand and gravel about 18 inches thick, and under it the same sort of blue clay as before, which continues to the depth of 48 feet from the surface. Below this there is a bed of dark-coloured stone, like ragstone, about six inches thick, from under which GEORGE NAYLOR says, that a salt spring issues. Beneath this layer of stone, there is a bed of dark-blue clay, which continues to the depth of 75 feet from the surface, where is a bed of stone, of a lightish colour, about 6 inches thick, and under it

a bed of dark-blue clay, which continues to the depth of 114 feet from the surface, where there is a bed of stone, of a brightish colour, about 8 inches thick, and under it a bed of gravel, about 6 inches thick, where GEORGE NAYLOR says there is another salt spring. Under the gravel, there is a bed of dark-coloured clay, resembling black-lead, which continues to the depth of 174 feet from the surface, when it changes to a chalky clay, intermixed with small pebbles and flints, which continues about 3 inches, and then changes to the same kind of dark-coloured clay as before; in which, after boring to the depth of 186 feet from the surface, he came to the solid earth bored to, in 1747, by the above-mentioned THOMAS PARTRIDGE. After boring in the same kind of clay to the depth of 210 feet from the surface, it changes to a lighter-coloured one, which continues about 6 inches, and then changes dark again, and continues so to the depth of 342 feet from the surface, where there is a bed of shells and white-coloured earth, about half an inch thick, and under it a light-coloured earth like that at 210 feet from the surface, and under it a bed of dark-coloured clay. After continuing in that clay to the depth of 444 feet from the surface, GEORGE NAYLOR put down a tin pipe, 50 yards in length, and 2½ inches in diameter within, to prevent the gravel and stones from falling down and obstructing the rods; but being too weak for that purpose, it separated into different lengths, and intirely prevented his boring, so that he was obliged to get the said pipes up again, which took him 48 days; having got them up, and cleared the hole pretty well, he left off boring until he could procure some stronger pipes.

In July, 1784, he put down 21 pipes of cast iron, which were cast at Chesterfield, in the county of Derby, each pipe

being 2½ inches in diameter within, half an inch thick, and upon an average 6 feet and 1 inch in length; they were affixed together with boxes and screws, and with a piece of soft leather between the top of each box and screw, to prevent them from breaking; the uppermost pipe is fastened to a plank, which lies upon the top of the brick-work.

At the distance of 447 feet from the surface there is a bed of dark-coloured earth mixed with chalk and gravel, which continues to the depth of 449 feet and 10 inches from the surface, where is a bed of dark-coloured earth without any chalk and with very little gravel, which continues to the depth of 454 feet and 7 inches from the surface; there it changes to a dark-coloured earth, mixed with chalk and gravel, which continues to the depth of 456 feet and 8 inches from the surface, and then changes to a dark-coloured earth without any chalk, and with very little gravel, which continues to the depth of 457 feet from the surface, and then changes of a lighter colour; and this continues to the depth of 462 feet and 4 inches from the surface, where it changes of a darker colour, and so continues to the depth of 470 feet and 3 inches from the surface. Here the ground changes to a dark-coloured earth, mixed with chalk and gravel, which continues to the depth of 470 feet and 7 inches from the surface, where he came to a bed of stone, like ragstone, about 13 inches thick, which ground into powder with the wimble, and mixed with the earth. Under this bed of stone there is a dark-coloured earth, without any chalk, and with but little gravel, which continues to the depth of 472 feet from the surface, when it changes something lighter, and continues so about 2 inches, where the earth appears to be mixed with chalk and gravel, and continues so for
about

about 1 inch, when it changes to a black silt, having a great deal of light-coloured sand in it.

On September the 6th, 1785, GEORGE NAYLOR broke one of the screws belonging to his rods just above the top of the box, at the distance of between 92 and 93 yards from the surface; when the upper rod, having a circular head or ring 2 inches in diameter at the top, dropped down 40 yards through the iron pipes; which rods were got up again on the 15th day of September by a spring. After trying several instruments to get up the lower part of the rods, to no effect, on the 3d day of October following was contrived a spiral instrument, about 2 feet long, with a catch at the top of it, to take the bottom of the uppermost box of the rods that were down; but the top of the rods having fallen several inches from the perpendicular, prevented the instrument from taking them between the first and second boxes: therefore, the Surveyor to the Corporation and the above-mentioned GEORGE NAYLOR, on the 7th day of October, contrived a spiral instrument, about 2 feet long, without any catch at the top, which GEORGE NAYLOR put down about 10 yards below the upper box, and there taking hold of the rods, raked them up to the top, and by that means brought them perpendicular, when he left them, and on the 8th day of October put down the instrument invented before; by which he got hold of the rods a little below the top box, and brought them up. When the rods broke, GEORGE NAYLOR was boring in a dark-coloured silt, intermixed with chalk and gravel, at the distance of 474 feet from the surface, which continued to the depth of 475 feet and 5 inches, when it changed to dark-coloured wet silt without any chalk, in which GEORGE NAYLOR bored to the depth of 478 feet and $8\frac{1}{2}$ inches from the surface. Here he imagined,
by

by the easy turning of the wimble, that he had got into a spring of water, and gave over boring, to see if the water would rise in the pipes ; when, after keeping the water in the well below the top of the pipes for several days (by pumping), the water in the pipes was found to rise about 5 feet *per* day upon an average ; which only producing about 7 pints, it was supposed there was no spring of water bored into, but that the rise of water in the pipes was occasioned by the foccage only.

On Monday the 28th day of November, an iron bucket was made and affixed to the bottom of the rods, and let down the pipes, and filled with water at the depth of 85 yards from the surface ; which water was salt, and of a reddish colour. The bucket was again let down, and filled at the depth of 156 yards from the surface ; that water was more salt than the first, and much of the same colour.

The Committee appointed by the Corporation for superintending the business of sinking for water, having taken the whole of these circumstances into their consideration, and examined GEORGE NAYLOR, who did not account, in a manner satisfactory to them, for the slow progress he had lately made in boring, were of opinion, that it would be proper for the present to discontinue all operations in the well ; they therefore directed the stage to be taken up, the mouth of the iron pipes to be carefully plugged up, the well to be covered with oak plank, and the ground over it to be paved as before ; all which was accordingly done.

Boston,
November 28, 1786.

JAMES LIMBIRD,
Surveyor to the Corporation.

XII. *Observations of Miss Herschel's Comet, in August and September, 1786. By the Rev. Francis Wollaston, LL.B. F. R. S.*

Read December 21, 1786.

THE comet of August last, having afforded me an opportunity of putting to some test the system of wires, a description of which I laid before this Society *, I think it may not be improper, as a sequel to that Paper, to give an account of the observations I have made with it on this occasion. Though, I believe, I have nearly ascertained with it the position of about 200 small stars in the Corona Borealis; yet such observations could not be examined by other gentlemen, without their going over the same ground. But this comet, which must have been observed by many, will serve to shew them, whether I have come near to the truth in this way; and what dependence may be had upon observations made with such an instrument.

The telescope to which I applied it was an achromatic object-glass of DOLLOND, of 16 inches focal length, and 2 inches aperture, with a RAMSDEN'S eye-glass, magnifying about 25 times, mounted on a very firm equatorial stand: with this, which takes in two degrees of a great circle, I compared the times of the comet and such stars as lay convenient, as they

* See vol. LXXV. p. 346.

severally

severally passed the centre wire and other adjoining wires; making occasionally a diagram, or drawing of their appearance in that telescope, in the manner represented in a Paper communicated to this Society in the year 1784 *. This I found of great service; both in ascertaining the stars of comparison, and in catching something of an observation on those evenings, which were not clear enough, or steady enough, for me to take the transits (as August 14. and 19. and Sept. 21.). In this way alone, one may be certain of the relative positions to five minutes of a degree in right-ascension or declination, under the most unfavourable circumstances, and commonly *much* nearer; and this occasionally has its use.

This comet was first discovered August 1, 1786, at Slough, near Windsor, by Miss CAROLINE HERSCHEL, Sister to Dr. WILLIAM HERSCHEL, and assistant to him, and almost as zealous an astronomer as himself; who, in his absence, swept the heavens with her telescope for that purpose. She observed it again August 2. and then communicated the news of it to her friends. Her account (as I recollect it, for I had not the original) was, that August 1. it was about equi-distant from ξ and ν Ursæ Majoris, and N° 14, 15, and 16. Comæ Berenices, about 1° south of the parallel of N° 15. Comæ. The news reached me on the 4th, which was cloudy; but on the 5th I saw the comet; and, having adjusted the wires, compared its passage with the adjoining stars; of which the following are the results. The times noted down are all sidereal; because they were so observed, and the reduction of them is most simple. The stars of comparison are mostly those of the British Catalogue: and to that the numbers refer; excepting a few, which I take to be stars to be found in the *Catalogue*

* See vol. LXXIV. p. 181.

complet d'Etoiles, par Bode, 1782; who, at the end of each constellation in FLAMSTEED's Catalogue, adds such stars, omitted by him, as have been observed by other astronomers, and carries on the numbers of each observer in succession. To them the name of the observer is here inserted. Those without any name or number are smaller stars, not in that or probably in any Catalogue.

Days.	At what hour fid.	Comet preceded or followed.	What star.	Differences	
				in R. A. fid. time.	in Decl.
1786	h.				
Aug. 5	19 27 40	Com. prec.	a * -	7 2	0 9 32 North of the star.
			N° 12. Com. Ber.	9 17	55 37 N.
			N° 16. -	13 45	
	19 51 57	Com. prec.	N° 13. -	11 4	
			N° 14. -	13 5,5	28 27 S.
			N° 16. -	13 43	2 26 S.
6	18 50 44	Com. prec.	a * -	0 5	N. about 1° 10'.
			a * -	0 6	27 11 N.
			N° 50 (Darquier)	0 26,5	2 26 S.
			N° 14. -	6 14	14 31 S.
			N° 16. -	6 49,5	12 2 N.
	19 9 23	Com. prec.	a * -	0 1	
			N° 50 (Darquier)	0 22,5	1 53 S.
			N° 13. -	4 1,5	
			N° 14. -	6 10	14 13 S.
			N° 16. -	6 44,7	12 36 N.
7	19 5 10	Com. foll.	N° 14. -	0 48	0 39 S.
			N° 16. -	0 12	26 14 N.
8	18 21 22	Com. foll.	N° 14. -	7 29	11 2 N.
			N° 16. -	6 53	37 52 N.
	18 35 52	Com. foll.	N° 14. -	7 33	11 22 N.
			N° 16. -	6 56,5	38 15 N.
	18 51 30	Com. foll.	N° 14. -	7 38,7	10 53 N.
			N° 16. -	7 3,4	37 49 N.
11	18 30 42	Com. foll.	N° 30. -	4 29,2	21 27 N.
			N° 31. -	2 5	22 7 N.
	18 38 15	Com. foll.	N° 30. -	4 31,7	21 25 N.
			N° 31. -	2 7,5	22 8 N.
	19 9 49	Com. foll.	N° 14. -	27 51	0 38 13 N.
			N° 16. -	27 15	1 4 0 N. Quære this Decl.

There is some error here, for which I cannot account.

Days.	At what hour & d.	Comet preceded or followed.	What star.	Differences	
				in R.A. sid. time.	in Decl.
1786	h.				
2 Aug. 11	19 9 49	Com. foll.	N ^o 30. —	4 41	21 34 N.
			N ^o 31. —	2 16	22 16 N.
14	19 6 0	Com. foll.	N ^o 43. about	0 50	22 0 N. <i>per diagram only.</i>
15	19 10 0	Com. foll.	a * Can. venat.	1 50	12 0 N.
			Nebula, N ^o 3.	0 20	0 20 S.
		Com. prec.	a small *	0 10	0 20 N.
	19 55 0	Com. foll.	a *	2 5	12 0 N.
			Nebula, N ^o 3.	0 35	0 20 S.
		Com. foll.	the small *	0 5	0 20 N.
20	18 44 2	Com. foll.	a * can. venat.	7 19	9 36 N.
			Nebula, N ^o 3.	5 51	9 13 S. <i>Quære this.</i>
			a *	3 48 :	
	18 55 29	Com. foll.	a *	7 23	9 55 N.
			Nebula, N ^o 3.	5 51	7 42 S.
			a *	3 52	38 13 N.
22	18 33 18	Com. foll.	N ^o 9. Bootis	2 20	39 6 N.
		Com. prec.	N ^o 11. —	2 20,5	46 26 N.
	18 45 38	Com. foll.	a *	5 42,5	31 37 S.
			a *	2 49	32 26 S.
			N ^o 9. —	2 23	38 22 N.
		Com. prec.	N ^o 11. —	2 17	45 48 N.
	18 58 18	Com. foll.	a *	5 45	30 34 S.
			a *	2 51	
			N ^o 9. —	2 26	39 26 N.
	19 8 31	Com. foll.	N ^o 9. —	2 29	39 31 N.
		Com. prec.	N ^o 11. —	2 11	47 3 N.
23	19 6 26	Com. foll.	N ^o 11. —	3 6	41 32 N.
24	19 2 22	Com. foll.	N ^o 9. —	13 1	28 49 N.
			N ^o 11. —	8 20	36 4 N.
			N ^o 72 (Darquier)	6 2	28 59 N.
			a *	2 28	26 32 S.
25	19 13 24	Com. foll.	N ^o 9. —	18 19	23 46 N.
			N ^o 11. —	13 38	31 5 N.
			a *	11 20	12 45 N.
			a *	8 22,5	10 49 N.
			a *	7 46,5	21 25 S.
		Com. prec.	a *	3 57	29 32 S.
			a *	9 19 :	41 9 N.
			N ^o 84 (Darquier)	9 26	32 43 N.

All these were only judged from a diagram.

In this set there is probably an error of 1' in the Decl. of the com.

Days.

Days.	At what hour sid.	Comet preceded or followed.	What star.	Differences	
				in R.A. sid. time.	in Decl.
1786	h.				
3 Aug. 25	19 27 45	Com. prec.	a *	3 56	30 51 N.
	19 27 45	Com. prec.	a small *	9 13,5	39 54 N.
			N°84 (Darquier)	9 24	31 50 N.
	19 43 39	Com. prec.	N°79 (Darquier)	0 40	10 53 N.
			a *	3 53	31 15 S.
3 - 29	19 20 27	Com. foll.	a *	1 56	
		Com. prec.	N°91 (Darquier)	2 13,5	0 44 S.
8 - 30	18 1 30	Com. foll.	N°91 (Darquier)	2 30	8 5 S.
		Com. prec.	Bootis -	6 24	19 0 N.
	18 15 49	Com. prec.	N°91 (Darquier)	2 27	7 36 S.
		Com. prec.	Bootis -	6 14	19 9 N.
24 - 31	19 18 24	Com. prec.	-	1 29,5	10 49 N.
	19 21 37	Com. prec.	-	1 30	11 6 N.
	19 24 41	Com. prec.	-	1 30 :	9 22 N.
2 Sept. 1	19 56 19	Com. foll.	-	3 12	0 26 N.
	20 5 50	Com. foll.	-	3 12	1 46 N.
6 - 2	19 47 7	Com. foll.	-	7 32	8 27 S.
		Com. prec.	a *	0 24	
	20 7 10	Com. foll.	-	7 36	8 40 S.
		Com. prec.	a *	0 17	29 14 N.
24 - 21	20 45 0	Com. foll.	π Serpente 1 ^{ma}	1 32	58 0 N. per diagram only.

The comet growing faint, I did not follow it any farther. During the whole time, it was invisible to the naked eye, and without any tail. Its appearance was so very similar to the nebula (N° 3. in MESSIER's Catalogue inserted in the *Connoissance des Temps* for 1784, and some other years) as scarcely to be distinguished from it when in the telescope together; though it certainly had a brighter spot in the centre.

The latter observations were not quite so satisfactory as the preceding; the comet growing faint. Those of the nebula, August 20. were also somewhat doubtful; the nebula not having the lucid point in the centre which the comet had; and therefore not being so easy to observe, especially in passing the oblique wires.

Upon the whole, I think, I may with confidence recommend such an instrument, as very convenient for sweeping the heavens, and pretty well ascertaining the position of what one discovers. I have here transcribed many observations which might better have been suppressed for the credit of the rest; but from them alone it is, that a judgement can be formed of the errors to which I was liable (which yet may possibly have been errors of calculation), and how far such an instrument is deserving of any farther recommendation. A larger telescope and greater magnifying power, to which I can apply these wires, certainly would have been capable of greater accuracy: but the field being smaller, the stars of comparison would have been the fewer, and the series of observations less connected. I have at other times used it with a DOLLOND'S eye-glass, with which it takes in a field of nearly three degrees; but the magnifying power is then so much the less, that, for this kind of observations, I prefer that of RAMSDEN, with which I can observe stars down to what I call the tenth or eleventh magnitude, and, I think, with some degree of precision.

Chislehurst, Nov. 18, 1786.



XIII. *An Account of a Thunder-storm in Scotland; with some Meteorological Observations. In a Letter from Patrick Brydone, Esq. F. R. S. to Sir Joseph Banks, Bart. P. R. S.*

Read January 18, 1787.

DEAR SIR,

Lancel-House, near Coldstream, Dec. 20, 1786.

I NOW fit down to give you some account of the thunder-storm, which I remember to have mentioned to you in conversation, and of which you wished to be more particularly informed. I do not know whether you will think it worthy of the attention of the Society; but you will be pleased to make whatever use of it you think proper. It is copied, with some additions, from the journal which I usually keep in the country, and which was wrote down immediately after the event.

Tuesday, the 19th of July, 1785, was a fine soft morning (thermometer, at ten, 68°); about eleven, clouds began to form in the south-east; and between twelve and one there were several flashes of lightning, followed by rolling claps of thunder, at a considerable distance. I was sitting in my study at an open window, in the second story, observing the progress of the storm; when some ladies, who were in the drawing-room below, alarmed by the lightning, came up to me. I was making them observe, by a stop-watch, the time which the sound took to reach us (which was generally from 25 to 30 seconds), and
 assuring

assuring them the storm was at so great a distance, that there could be no sort of danger; when we were suddenly alarmed by a loud report, for which we were not prepared by any preceding flash: it resembled the firing of several muskets, so close together, that the ear could hardly separate the sounds; and was followed by no rumbling noise like the other claps.

The clouds immediately began to dissipate, and there were no more appearance of either thunder or lightning. I had ordered my horses to be got ready, and was just going to mount, when a servant came running in to tell me, that a man and two horses had been struck dead by the thunder, at a small distance from the house. I immediately set out, and arrived on the spot in less than half an hour after the accident. The horses were still yoked to the cart, and lying in the same position in which they had been struck down; but the body of the young man had been already carried off by his companion, who soon returned to the place; and, with less agitation than I expected, described to me how every thing had passed.

They were both servants to Mr. TURNBULL, a tenant of the Earl of HOME, and were returning home with two carts loaded with coals. JAMES LAUDER, a strong young man, of twenty-four years of age, had the charge of the first cart, and was sitting on the fore-part of it. They had crossed the Tweed a few minutes before, at a deep ford, and had almost gained the highest part of an ascent, about 65 or 70 feet above the bed of the river. They were conversing about the thunder, which they heard at a distance, and expressing a wish that it might be accompanied by a fall of rain, as the only means of saving the crop, after so long and so severe drought. At that instant he was stunned by a loud report, and saw his companion, his horses and cart, fall to the ground. He immediately

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ran

ran to his assistance, but found him quite dead. His face, he said, was of a livid colour, his cloaths were torn to pieces, and he had a strong smell of burning. He immediately emptied his own cart, and carried home LAUDER's body to his friends; so that I had not an opportunity of examining it: but Mr. BELL, Minister of Coldstream, a gentleman of the most perfect candour and veracity, told me, that he had been sent for, to announce the fatal event to the young man's parents, and had examined the body; that he found the skin of the right thigh much burnt and shrivelled, and many marks of the same kind over the whole body; but none on the legs, which he imputed to their hanging over the fore-part of the cart at the time of the explosion, and not being in contact with any part of it. His cloaths, and particularly his shirt, was very much torn, and emitted a strong smell of burning. The body was buried two days after, without having discovered any symptoms of putrefaction.

LAUDER's companion shewed me the distance between the two carts, which was exactly marked; for his horses had turned round at the time of the explosion, and broke their harness: I found it about twenty-four yards, and LAUDER's cart was a few feet higher on the bank, but had not yet reached the summit. He told me, he was likewise sitting on the fore-part of his cart, and had LAUDER, his cart and horses, full in view, when they fell to the ground; that he perceived no flash, nor appearance of fire, and was sensible of no shock, nor uncommon sensation.

I now examined the cart, and the spot around it, as exactly as I could. The horses were black, and of a strong make; they had fallen on the left side, and their legs had made a deep impression in the dust, which, on our lifting them up, shewed

shewed the exact form of each leg ; so that no kind of struggle or convulsive motion had succeeded the fall, but every principle of life seems to have been extinguished in an instant. The hair was much singed over the greatest part of their bodies ; but was most perceptible on the belly and legs. Their eyes were already become dull and opake, and looked like the eyes of an animal which had been long dead. The joints were all supple ; and I could not perceive that any of the bones were either softened or dissolved, as it has been alledged sometimes happens to animals killed by lightning. The left shaft of the cart was broken ; and I observed, that splinters had been thrown off in many places, particularly where the timber of the cart was connected by nails, or cramps of iron. Many pieces of the coal were likewise thrown out to a considerable distance, all round the cart ; and some of them, which I have preserved, have the appearance of coal which had lain some time on a fire. I likewise gathered up the fragments of LAUNDER's hat, which had been torn to innumerable small pieces ; some of which I shall inclose for your inspection, as well as part of his hair, which I found strongly united to some of the fragments which had composed the crown of the hat *. About four feet and a half behind each wheel of the cart, I observed an odd appearance in the ground ; a circular hole of about twenty inches in diameter, the center of which was exactly in the tract of each wheel. The earth was torn up, as if by violent blows of a pick-axe, and the small stones and dust were scattered on each side of the road. The tracks of the wheels were strongly marked in the dust, both behind and before these holes, but were completely obliterated for upwards of a foot and a half on these spots. This led me to suspect, that the force which had formed them must likewise have

* These were exhibited to the Society. C. B.

acted strongly upon the wheels; and, on examination, I found evident marks of fusion on each of them, which I now shewed to many people who had assembled around us. The surface of the iron, to the length of about three inches, and the whole breadth of the wheel, had become of a bluish colour, had entirely lost its polish and smoothness, and had the appearance of drops incompletely formed on its surface; these were of a roundish form, and had a sensible projection. I suspected that the great heat, which had been communicated to the iron, might probably have burnt the wood of the wheels; but this I did not find to be the case. To ascertain whether these marks were occasioned by the explosion which had torn up the ground, we pushed back the cart on the same tracks which it had described on the road; and found, that the marks of fusion answered exactly to the center of each of the holes; and that, at the instant of the explosion, the iron of the wheels had been sunk deep in the dust. They had made almost half a revolution after the explosion, which might be occasioned by the falling down of the horses, which pulled the cart a little forward. On examining the opposite part of the wheels, or that part which was at the greatest distance from the earth, no mark of any kind was observable. The broken earth still emitted a smell something like that of ether. The ground was remarkably dry, and of a gravelly soil.

It would appear, that this great explosion had, in an instant, pervaded every substance connected with the cart, the wheels of which had probably conducted it from the ground. They had been completely wetted but a few minutes before, as well as the legs and bellies of the horses, and this might, perhaps, be the reason why the hair on these parts was much more burnt than on the rest of their bodies. However, the two horses

had already walked over this electrical mine, without having produced any effect; and had not the cart followed them might have escaped without hurt. I examined all their shoes, but could not perceive the least mark on any of them, nor was the earth broken where they had trodden. But the cart was deeply laden, and the wheels had penetrated much farther into the ground.

The equilibrium between the earth and the atmosphere seems at this instant to have been completely restored; for no farther appearance of thunder or lightning was observed within our hemisphere; the clouds dispelled, and the air resumed the most perfect tranquillity: but how this vast quantity of electric matter could be discharged from the one element into the other without exhibiting any appearance of fire, I shall not pretend to examine. The fact, however, appears certain; and when I was mentioning it as a singular one, a gentleman told me, that the shepherd of St. Cuthbert's farm, on the opposite bank of the Tweed, had been an eye-witness of the event, and gave a different account of it. I immediately went to the farm, found the shepherd, and made him conduct me to the spot from whence he had observed it, and desired him to give me an account of what had happened. He was looking, he said, at the two carts going up the bank, when he was stunned by a loud report, and at the same instant saw the first of the carts fall to the ground, and observed that the man and horses lay still, as if dead. I asked him, if he had observed any lightning? He said, he saw no lightning, nor appearance of fire whatever; but observed the dust to rise at the place; that there had been several flashes of lightning some time before from the south-east, whereas the accident happened to the north-west of where he stood. The distance, in a right line
across.

across the river, might be between two and three hundred yards. He was sensible of no shock, nor uncommon sensation of any kind. I went next morning to examine if there were any marks of putrefaction on the horses, and to observe the state of the blood-vessels, &c. after the skin had been taken off; but a gentleman of the neighbourhood, who kept a pack of hounds, had already seized on them.

Several other phænomena happened on that day, probably all proceeding from the same cause; some of which I shall beg leave to mention.

The shepherd, belonging to the farm of Lennel-Hill, was in a neighbouring field, tending his flock, when he observed a lamb drop down; and said, he felt at the same time as if fire had passed over his face (this was his own expression), although the lightning and claps of thunder were then at a great distance from him. He ran up immediately, but found the lamb quite dead; nor did he perceive the least convulsive motion, nor symptom of life remaining, although the moment before it appeared to be in perfect health. He bled it with his knife, and the blood flowed freely. This, he told me, happened about a quarter of an hour before the explosion which killed LAUDER; and it was not above three hundred yards distant from the spot. He was only a few yards from the lamb when it fell down. The earth was not torn up, nor did he observe any dust rise.

THOMAS FOSTER, a celebrated fisher in Coldstream, and another man, were standing in the middle of the Tweed, fishing for salmon with the rod, when they suddenly heard a loud noise; and, turning round to see from whence it came, they found themselves caught in a violent whirlwind, which FOSTER told me felt sultry and hot, and almost prevented

them from breathing. It was not without much difficulty they could reach the bank, where they sat down, exhausted with fatigue, and greatly alarmed: however it lasted but a very short time, and was succeeded by a perfect calm. This happened about an hour before the explosion.

A woman, making hay near the banks of the river, fell suddenly to the ground, and called out to her companions, that she had received a violent blow on the foot, and could not imagine from whence it came. This I had not from the woman herself, but from Mr. TURNBULL, a very respectable farmer. Mr. BELL, our minister, nephew of THOMSON the Poet, and possessed of all the candour and ingenuity of his uncle, told me, that, walking in his garden, a little before LAUDER's accident, he several times felt a sensible tremor in the ground. He likewise told me (what I find I had forgot to mention in the proper place), that he had observed on LAUDER's body a zig-zag line, of about an inch and a quarter broad, which extended from his chin down to his right thigh, and had followed nearly the line of the buttons of his waistcoat. The skin was burnt white and hard.

These, sir, are all the circumstances I have been able to collect that are well authenticated; and I shall not trouble you with reports that are not. From the whole it would appear, that the earth had acquired a great super-abundance of electrical matter, which was every where endeavouring to fly off into the atmosphere. Perhaps it might be accounted for from the excessive dryness of the ground; and, for many months, the almost total want of rain, which is probably the agent that Nature employs in preserving, or in restoring, the equilibrium between the other two elements. But I shall not pretend to investigate the causes: all I wanted, was to give you some
account

account of the effects; and your own reflections will lead you much farther than any thing I could suggest.

I have the honour to be, with the greatest respect, &c.

P. BRYDONE.

P.S. I cannot send away this letter without adding, in a Post-script, that on Friday the 11th of August last, early in the morning, we had a pretty smart shock of an earthquake. I was awaked by it, and felt the motion most distinctly for four or five seconds at least. It appeared as if the bed had been pulled gently from side to side several times. The motion was nearly north north-west and south-east, as far as I could judge from the motion of the bed. The windows were violently shaken, and made a great noise, which, I believe, was mistaken by many people for a noise accompanying the earthquake. I immediately rose to look at my watch, and found it twenty minutes after two. It was a dead calm, the morning close and warm, with small drizzling rain, and, although the moon was but two days past the full, so dark that I could not perceive the hour without striking a light. It was felt in almost every house in this neighbourhood, and all the way from this country to the west coast of the island, where it seems to have been more violent than here; but to the east of this place it was very little felt.

Perhaps it may not be improper to mention the state of the weather for some time before and after this event, as it may possibly have had some influence upon it. The drought was very great till the 22d of July, when it rained a little; and this was repeated, though in small quantities, and generally accompanied by high winds, till Thursday the 27th, when it blew.

blew the most violent tempest I ever remember in this country. The young crop of turneps, in many fields, were blown out of the ground, and almost entirely destroyed. The pease became brown as if withered, and so did the leaves of the forest trees on that side which was opposed to the blast. Vast clouds of dust were raised from the dry fields and roads, which looked like smoke, and had the appearance at a distance as if many villages had been on fire all over the country. The water too was raised from the surface of the river, and carried quite away by the violence of the hurricane, forming small clouds in the air, which we traced to a great distance. The great violence of this tempest lasted but a few hours, and at night it fell calm. The barometer was little affected, and stood at 29 inches and a half. The wind was nearly west, veering sometimes a little to the north. From this time we had a tract of very fine weather, the wind constantly in the west points, till the time of the earthquake (which happened on what is called the last of the dog days), when it changed to the south-east, and brought us five of the worst days I ever remember to have seen at that season; it rained almost incessantly, with a cold easterly wind, and the sun did not once appear till the morning of Wednesday the 16th, after which we had again a tract of fine weather. I examined the barometer at the time of the earthquake, but did not find that it had been sensibly affected. It rose a little on that morning; but this I imputed to the wind having changed into the east.



XIV. *On finding the Values of Algebraical Quantities by converging Serieses, and demonstrating and extending Propositions given by Pappus and others.* By Edward Waring, F.R.S. Professor of Mathematics at Cambridge.

Read February 8, 1787.

SUPPOSE the roots of the equation $x^b \pm 1 = 0$ to be given, where b denotes any whole number or fraction; to find the roots or values of any given algebraical quantity, by converging infinite serieses.

1. Let the algebraical quantity be $\sqrt[n]{(\pm A)}$, then the roots of the algebraical quantity will be $A^{\frac{1}{n}} \times (\alpha + \lambda \sqrt{-1})$, $A^{\frac{1}{n}} \times (\beta + \mu \sqrt{-1})$, $A^{\frac{1}{n}} \times (\gamma + \nu \sqrt{-1})$, &c. where $\alpha + \lambda \sqrt{-1}$, $\beta + \mu \sqrt{-1}$, $\gamma + \nu \sqrt{-1}$, &c. are the roots of the equation $x^b \pm 1 = 0$; it will be $+1$ if it was $-A$, and -1 if $+A$.

2. Let the given algebraical quantity be $\sqrt[n]{(\pm \sqrt[n]{(\pm A)} \pm \sqrt[n]{(\pm B)} \pm \sqrt[n]{(\pm C)} \pm \&c.)}$, and $\alpha + \lambda \sqrt{-1}$, $\alpha' + \lambda' \sqrt{-1}$, $\alpha'' + \lambda'' \sqrt{-1}$, &c. and $\Gamma + \Delta \sqrt{-1}$ be respectively one of the roots of the equations $x^n \mp 1 = 0$, $x^m \mp 1 = 0$, $x^p \mp 1 = 0$, &c. and $x^r \mp 1 = 0$; substitute $\pm P = \pm A^{\frac{1}{n}} \alpha \pm B^{\frac{1}{m}} \alpha' \pm C^{\frac{1}{p}} \alpha'' \pm \&c.$ and $\pm Q = \pm A^{\frac{1}{n}} \lambda \pm B^{\frac{1}{m}} \lambda' \pm C^{\frac{1}{p}} \lambda'' \pm \&c.$ In the first place let P be greater Q , and $\pm P$ be $+P$, then will $(P \pm Q \sqrt{(-1)})^{\frac{1}{r}} =$
(P

$$\begin{aligned}
 & \left(P^{\frac{1}{r}} - \frac{1}{r} \cdot \frac{1-r}{2r} \times \frac{Q^2}{P^{\frac{2r-1}{r}}} + \frac{1}{r} \cdot \frac{1-r}{2r} \cdot \frac{1-2r}{3r} \cdot \frac{1-3r}{4r} \times \frac{Q^4}{P^{\frac{4r-1}{r}}} - \&c. \right. \\
 & \left. = \pm L \right) \pm \left(\frac{1}{r} \cdot \frac{Q}{P^{\frac{r-1}{r}}} - \frac{1}{r} \cdot \frac{1-r}{2r} \cdot \frac{1-2r}{3r} \cdot \frac{Q^3}{P^{\frac{3r-1}{r}}} + \frac{1}{r} \cdot \frac{1-r}{2r} \cdot \right. \\
 & \left. \frac{1-2r}{3r} \cdot \frac{1-3r}{4r} \cdot \frac{1-4r}{5r} \times \frac{Q^5}{P^{\frac{5r-1}{r}}} - \&c. = \pm M \right) \times \sqrt{-1} = \pm L \pm M
 \end{aligned}$$

$\sqrt{-1}$, in which case the two serieses $\pm L$ and $\pm M$ converge, and $(\Gamma + \Delta \sqrt{-1}) \times (\pm L \pm M \sqrt{-1})$ will be a value or root of the given quantity.

In the same manner the remaining roots may be deduced.

2. Let $\pm P$ be $-P$, multiply $-P \pm Q \sqrt{-1}$ into -1 , and it becomes $P \mp Q \sqrt{-1}$ a quantity of the same formula as the preceding; let $\Gamma' + \Delta' \sqrt{-1}$ be a root of the equation $x^r + 1 = 0$, then will $(\Gamma' + \Delta' \sqrt{-1}) (\pm L \pm M \sqrt{-1}) = \pm H' \pm K' \sqrt{-1}$ be a root of the given quantity: otherwise; the root may be deduced from the above-mentioned series by substituting in it for $-(P)^{\frac{1}{r}}$ its value $P^{\frac{1}{r}} \times (-1)^{\frac{1}{r}}$, and it will become the same as the preceding.

3. Let P be less than Q , and the value of $(\pm P \pm Q \sqrt{-1})^{\frac{1}{r}}$ may be deduced from the preceding series by substituting in it $\pm Q \sqrt{-1}$ for P , and $\pm P$ for Q : otherwise, since $(\pm P \pm Q \sqrt{-1})^{\frac{1}{r}} = \pm \sqrt[r]{{-1}} \times (Q \mp P \sqrt{-1})^{\frac{1}{r}}$, and the root of $(Q \mp P \sqrt{-1})^{\frac{1}{r}}$ can be deduced by the preceding method, which suppose $L' + M' \sqrt{-1}$; multiply this root into $H \pm \Theta \sqrt{-1}$, where $H + \Theta \sqrt{-1}$ denotes a value of the root $\pm \sqrt[r]{{-1}}$, and the quantity resulting will be

be one value of the given quantity; the remaining values can be deduced by the same method.

In this case the given quantity is resolved into a series ascending according to the dimensions of P , and descending according to the dimensions of Q ; in the former case it was resolved into a series ascending according to the dimensions of Q , and descending according to the dimensions of P ; both the series affording the possible or impossible parts will always converge.

4. If $P = \pm Q$, then will $(\pm P \pm P\sqrt{-1})^{\frac{1}{r}} = P^{\frac{1}{r}} \times (\pm 1 \pm \sqrt{-1})^{\frac{1}{r}} = P^{\frac{1}{r}} \times 2^{\frac{1}{2r}} (\pm \sqrt{\frac{1}{2}} \pm \sqrt{-\frac{1}{2}})^{\frac{1}{r}} = P^{\frac{1}{r}} \times 2^{\frac{1}{2r}} \times \sqrt[4]{-1}$; for $\sqrt[4]{-1} = \pm \sqrt{\frac{1}{2}} \pm \sqrt{-\frac{1}{2}}$.

4. 2. When $P = 0$, or $Q = 0$, then it becomes the first case $\sqrt[4]{(\pm A)}$.

5. Let $P = Q \mp \alpha$, where α has a very small ratio to Q ;

then will $(P \pm Q\sqrt{-1})^{\frac{1}{r}} = (P \pm \overline{P \pm \alpha}\sqrt{-1})^{\frac{1}{r}} = (P \times 2^{\frac{1}{2}} \times \overline{-1^{\frac{1}{2}}} \pm \alpha\sqrt{-1})^{\frac{1}{r}} = P^{\frac{1}{r}} \times 2^{\frac{1}{2r}} \times \sqrt[4]{-1} \pm \frac{1}{r} \times P^{\frac{1-r}{r}} \times 2^{\frac{1-2r}{2r}} \times \frac{4r}{1-2r} \sqrt{-1} \times \sqrt{-1} \alpha - \frac{1}{r} \times \frac{1-2r}{2r} \times P^{\frac{1-2r}{r}} \times 2^{\frac{1-2r}{2r}} \times \frac{4r}{1-3r} \sqrt{-1} \times \alpha^2 \pm \frac{1}{r} \times \frac{1-2r}{2r} \times \frac{1-2r}{3r} \times P^{\frac{1-3r}{r}} \times 2^{\frac{1-3r}{2r}} \times \frac{4r}{1-3r} \sqrt{-1} \times \sqrt{-1} \alpha^3 + \&c.$ In this series the same root of the quantity $\sqrt[4]{-1}$ is always to be used.

6. If in the given quantity are contained more quantities of the above-mentioned kind or their roots; then, by repeating the same operation, can be deduced the roots or values of the given quantity.

In some cases the impossible part may vanish, which may be the case in a quantity of the following formula, *viz.*

$\sqrt[2m]{a + \alpha \sqrt[2m]{-b}} + \sqrt[2m]{a + \beta \sqrt[2m]{-b}} + \sqrt[2m]{a + \gamma \sqrt[2m]{-b}} + \&c.$ where α , β , γ , &c. denote the $2m$ roots of $\sqrt[2m]{-1}$. The general principles of discovering the cases in which this happens have been given in the *Meditationes Algebraicæ*.

The roots of the equation $x^b \pm 1 = 0$ will be found from common algebra and these principles, if b is not greater than 10; or, more generally, if $b = 2' \times 3' \times 4' \dots 10'^8$, where l , l' , $l'' \dots l'^8$ denote any whole numbers: or, in general, the roots of the above-mentioned equation, or even of the equation $x = \sqrt[2m]{\pm L \pm M \sqrt[2m]{-1}}$, can be found from tables of sines.

The same principles may be applied to the discovery of the values of exponential irrational quantities.

In the *Miscel. Analy.* was given, from a substitution invented by me and not similar to any before given, a resolution of equations, which contains the resolutions of all equations before given, and from which the resolutions of some equations, not before delivered, have been added.

Part II. 1. Let an equation $A = 0$ involving (r) unknown independent quantities be predicated of another equation containing the same quantities, and the demonstration of it be required.

1st. Reduce both the equations to equations involving independent quantities only; then reduce the two equations to one, so that one of the above-mentioned quantities may be exterminated, and if there results a self-evident equation, *viz.* $A = A$, or $A - A = 0$, in which the correspondent terms destroy each other respectively; then the first equation is justly predicated of the second; that is, if the above-mentioned equations

equations afford the same value of the quantity exterminated, the proposition is true, otherwise not.

Cor. From these principles can be demonstrated many propositions given by PAPPUS and others.

Ex. Let $AD=2AC=2x$, $DE=a$, and $EB=b$, where AD , DE , and EB , are independent quantities; if $AB \times BE = (2x+a+b) \times b = CB \times BD = (x+a+b)(a+b)$, then will $CB=x+a+b : BD=a+b :: AC \times CE = x \times (x+a) : AD \times DE = 2x \times a$. From hence can be deduced the two equations $(b-a)x = a^2 + ab$ and $2a \times (x+a+b) = (a+b) \times (x+a)$; reduce these two equations to one, so as to exterminate x , and there results the self-evident equation $(a-b) \times \frac{a^2+ab}{b-a} (-a^2-ab) + a^2+ab=0$, and consequently the proposition is true.

2. If (s) equations involving $(t+r)$ unknown and independent quantities be predicated of (t) equations involving the above-mentioned quantities: reduce the (t) equations and one of the above-mentioned (s) equations to one, so that (t) unknown quantities may be exterminated, and if there results a self-evident equation, then the above-mentioned equation is justly predicated of the (t) equations; and in the same manner we may reason concerning the remaining $(s-1)$ equations.

3. 1. If one equation is justly predicated of another, and in both the unknown quantity exterminated has only one dimension; then the latter equation can be predicated of the former; for in this case both equations have only one and the same value of the unknown quantity exterminated.

3. 2. If the quantity exterminated has more dimensions than one in the equations, then the proposition may not generally

rally be true; for the equations may have some roots the same, but not all.

These observations may be applied to more equations.

4. From (n) given equations $a=0$, $b=0$, $c=0$, &c. can easily be deduced others dependent on them, by finding any direct algebraical functions of the above-mentioned equations, that is, $\phi(a, b, c, \&c.)$, which will always $=0$; and in like manner, from the relation between any lines being given, can be deduced innumerable relations between the above-mentioned lines, and other lines dependent on them.

Part III. 1. Ratios, which are supposed greater or less than others, can easily be transformed into equations, which contain affirmative and negative quantities: for example, let the ratio $a : b$ be greater than the ratio $c : d$, then will $\frac{b}{a} = \frac{c}{d} - k$; if it be less, then will $\frac{b}{a} = \frac{c}{d} + k$, where k denotes an affirmative quantity; and, *vice versa*, if $\frac{b}{a} = \frac{c}{d} - k$, then will the ratio of $a : b$ be greater than the ratio of $c : d$, &c.

2. If one quantity (a) is affirmed to be greater than another b , for a in the given equations substitute its value $b+k$; if less, for a write $b-k$, where k denotes an affirmative quantity.

3. Reduce the equations, so as to take away their denominators, and the demonstration of the proposition will often very easily follow.

4. Let $k = \frac{P}{Q}$ and $k' = \frac{P'}{Q'}$; and if P and Q be affirmative, let P' and Q' be affirmative; and, *vice versa*, if negative, negative; then, if k be affirmative, will k' also be affirmative; the same also may be affirmed, if P and Q have both contrary

trary signs to P' and Q' ; but if one has the same, and the other contrary, then will k and k' have contrary signs.

5. Let some affirmative quantities be less than others, then any direct affirmative function of the former, viz. function, in which no negative or impossible quantities or indexes are contained, will be less than the same function of the latter. The contrary happens when the indexes are all negative, and the quantities affirmative as before: for example, let two quantities be less than two others, then the product of the two former will be less than the product of the two latter.

Cor. Hence some quantities may often be known to be greater or less than others from their direct functions being greater or less than the same functions of the others: for example, let $a^2 - b^2$ be an affirmative quantity, then will a be greater than b .

6. If one equation or ratio is affirmed on the supposition that another given one is true, reduce both the equations by the methods given above, and from the principles before delivered, the proposition will often be evident.

Hence may be deduced demonstrations to propositions of this sort given by PAPPUS and others.

Ex. Let the ratio $a + b : b$ be greater than $c + d : d$, then the ratio $b : a - b$ will be less than $d : c - d$.

For, since the ratio $a + b : b$ is greater than $c + d : d$, the ratio $b : a + b$ will be less than $d : c + d$, and consequently $\frac{a+b}{b} \left(\frac{a}{b} + 1 \right) = \frac{c+d}{d} \left(\frac{c}{d} + 1 \right) + k$, whence $\frac{a}{b} - 1 = \frac{c}{d} - 1 + k$, and $\frac{a-b}{b} = \frac{c-d}{d} + k$, and the ratio $b : a - b$ less than $d : c - d$.

Ex. 2. Let the ratio of $a + b : c + d$ be greater than the ratio of $a : c$, then will the ratio of $b : d$ be greater than the ratio of $a + b : c + d$. By the preceding method convert these ra-

tios

tios into equations, and there result $\frac{c+d}{a+b} + k = \frac{c}{a}$ and $\frac{d}{b} + k' = \frac{c+d}{a+b}$; and the proposition asserts, that if k be an affirmative quantity, k' will also be an affirmative quantity. Reduce these two equations, so as to take away their denominators, and the resulting equations will be $ac + ad + a \times \overline{a+b} \times k = ac + bc$ and $ad + bd + \overline{a+b} \cdot bk' = bc + bd$, whence $k = \frac{bc-ad}{a(a+b)}$ and $k' = \frac{bc-ad}{b(a+b)}$, and the proposition is evident.

Ex. 3. Let a be greater than c , and b , and $(a+b) \times (a-b) = (c+d) \times (c-d)$, that is, $a^2 - b^2 = c^2 - d^2$, then will b be greater than d ; for a in the equation $a^2 - b^2 = c^2 - d^2$ write $c+k$, and there results $2ck + k^2 = b^2 - d^2$, whence $b^2 - d^2$ is an affirmative quantity, and consequently b greater than d .

Ex. 4. Let, as in Ex. 1. the ratio $a+b : b$ be greater than $c+d : d$, then will $b : a-b$ be less than the ratio $d : c-d$. By the preceding method translate these ratios into the two equations $\frac{b}{a+b} + k = \frac{d}{c+d}$ and $\frac{a-b}{b} = \frac{c-d}{d} + k'$, reduce these equations, so as to take away their denominators, and there result $bc + bd + \overline{a+b} \times \overline{c+d} k = ad + bd$ and $da - db = bc - bd + bdk'$, and consequently $k = \frac{ad-bc}{(a+b)(c+d)}$ and $k' = \frac{ad-bc}{bd}$; but these two fractions which express the values of k and k' have the same numerators, and their denominators both affirmative; therefore, if one k be affirmative, the other k' will also be affirmative.

Cor. From these principles can easily be deduced innumerable propositions of this sort. Assume two or more ratios, of which let some be supposed greater than others; then, from the above-mentioned transformation, by addition, subtraction, multiplication, division, &c. can be found such functions of the

the above-mentioned quantities, that some may become greater than others, and thence may be deduced the propositions above-mentioned.

7. It may not be improper in this place to adjoin a few observations on finding the limits of some quantities in which others contained in given equations become negative or affirmative.

1. Given an equation involving two unknown quantities x and y ; the limits of the quantity y , between which the quantity x will become affirmative or negative, may be deduced from the following principles.

The quantity x passes from affirmative to negative or from negative to affirmative, either through nothing or infinite; or from two impossible roots it passes to affirmative or negative through two or more equal roots; and, *vice versa*, from affirmative or negative to two or more impossible roots through two or more equal roots.

Find therefore the values of y , when x becomes $=0$, or infinite; and also all the cases in which two, &c. values of x become equal, that is, when its roots become impossible; and from thence can be deduced the limits of the quantity y , between which (x) becomes affirmative or negative.

2. If $x = \frac{P}{Q}$ be an affirmative quantity, then P will be affirmative or negative, according as Q is an affirmative or negative quantity, &c. Assume therefore $P=0$ and $Q=0$, and from the roots of the resulting equation can be deduced the cases, in which (x) becomes an affirmative quantity.

3. If more (n) unknown quantities (x, y, z, v , &c.) be contained in a given equation; then, by the preceding method, find the limits of (z, v , &c.), between which (x) becomes an affirmative or negative quantity, and let the quantities denoting the limits contain not more than ($n-1$) unknown quantities:
from

from the above-mentioned quantities or equations expressing the limits, find others denoting their limits, which do not contain more than $(n - 2)$ above-mentioned quantities, and so on.

4. Often from the substitution of the limits of given quantities can be acquired the limits of the remaining one (x). Find all the greatest values of the quantity (x) contained between the above-mentioned limits, and thence can be deduced the limits sought.

5. If there are given (m) equations involving $(m + 1)$ or more unknown quantities; then sometimes with, and sometimes without, reducing them to others involving more few unknown quantities can be found by the preceding method limits; and from comparing the limits so acquired can sometimes be deduced the limits sought.

6. If a given function of the unknown quantities (x, y, z , &c.) is asserted to be contained between given limits, when other functions of the above-mentioned quantities are contained between given limits, and the demonstration of it is required; from the given equations and the given functions find limits of the unknown quantities respectively, and if the latter limits are contained between the former, the proposition is generally true, otherwise not.

7. From the above-mentioned principles can be found the cases in which an unknown quantity (x) admits of one or more affirmative values.

8. It appears from the principles before delivered, that the finding the number of affirmative and negative roots of a given equation necessarily includes the finding the number of its impossible roots; and therefore it may not be improper to subjoin somewhat on what has been done on this subject.

1. DESCARTES

1. DESCARTES gave a method of finding the number of affirmative and negative roots of a given equation, when all its roots are possible; but all the roots are very seldom in equations of superior dimensions possible, unless when the equation is purposely made.

2. It has been demonstrated by others and myself, that the equation will at least have so many changes of signs from + to -, and - to +, as there are affirmative roots, and so many continued progresses from + to + and - to -, as there are negative roots.

3. A rule for finding in general the number of affirmative or negative roots in a biquadratic, and in the equation $x^4 + Ax^3 + B = 0$, was first published in the *Medit. Algebr.*

4. HARRIOT demonstrated a method of finding the number of impossible roots contained in a cubic equation. In the year 1757 I sent to the Royal Society a method of finding the number of impossible roots contained in a biquadratic and quadrato-cubic equations, and in the equation $x^4 \pm Ax^3 \pm B = 0$.

5. SCHOOTENGAVE a method of finding the number of impossible roots which can be concluded from the deficient terms of an equation. NEWTON gave a rule which often discovers the number of impossible roots contained in a given equation. CAMPBELL discovered a new rule on the same subject. Mr. MACLAURIN has added somewhat more general on these subjects: these rules may be rendered more general by a principle first given in the *Miscell. Analyt. viz.* multiplying the given equation into a quantity $x - a$ or $(x - a) \times (x - b)$, &c. and finding from the rule the number of impossible roots contained in the given equation. Similar and more general rules and principles have been added in the *Medit. Algebr.* These rules, in equations of superior dimensions, seldom discover the true number of im-

possible roots. I believe also, that I first gave a rule in the *Miscell. Analyt.* for finding the number of impossible roots from finding an equation, whose roots are the squares &c. of the roots of a given equation, which rule in equations of superior dimensions sometimes finds impossible roots, when NEWTON's, CAMPBELL's, &c. rules fail, and fails when they find them; and also a rule for finding impossible roots from an equation, whose roots are the squares of the differences of the roots of the given equation; this rule (as has been observed by me in the *Miscell. Analyt.* and *Philosophical Transactions*) always discovers whether all the roots of the given equation are possible or not; and the last term of the resulting equation discovers also, whether 0, 4, 8, 12, &c. or 2, 6, 10, 14, &c. impossible roots, are contained in the given equation; to which may be subjoined, if the given equation has r possible and $n - r = 2t$ impossible roots, that the number of changes of signs from + to - and - to + in the resulting equation will not be less than $r \cdot \frac{r-1}{2}$, and the number of continued progresses from + to + and - to - will not be less than t : whence, if the number of continued progresses be t' , the number of impossible roots will not be greater than $2t'$, and the number of possible roots not less than $n - 2t'$. If the number of changes of signs be b' , the number of possible roots will not be greater than r' , where $r' \times \frac{r'-1}{2}$ is the greatest possible number which does not exceed b' , and the number of impossible roots not less than $n - r'$. Another rule was, I believe, first given by me in the *Miscell. Analyt.* 1762, for finding impossible roots by finding an equation whose roots are x , where $x^n - px^{n-1} + qx^{n-2} - \&c. = z$, and $nx^{n-1} - n - 1 px^{n-2} + n - 2 qx^{n-3} - \&c. = 0$.

In

In the *Medit. Algebr.* somewhat has been added concerning impossible, affirmative, and negative values of the unknown quantities in an equation which involves two or more unknown quantities; and also was first delivered a rule from the number of affirmative, negative, and impossible roots of an equation being known to find the number of impossible, negative, and affirmative roots of an equation, whose roots have a given algebraical relation to the roots of a given equation; on which two last subjects little, I believe, had been before published.



XV. *Experiments on the Production of Dephlogisticated Air from Water with various Substances. In a Letter from Sir Benjamin Thompson, Knt. F. R. S. to Sir Joseph Banks, Bart. P. R. S.*

Read Feb. 15, 1787.

D E A R S I R,

Munich, Sept. 1, 1786.

VARIOUS opinions having been entertained with respect to the origin of the dephlogisticated air, produced by exposing healthy vegetables in water to the action of the sun's rays, according to the method of Dr. INGEN-HOUSZ; and not being myself thoroughly satisfied with any of the theories proposed, I made the following experiments, with a view to throwing some new light upon that subject.

Having found that raw silk possesses a power of attracting and separating air from water in great abundance, when exposed in it to the action of light, it occurred to me to examine the properties of this air, and to consider more attentively the circumstances attending its production, thinking that this might possibly lead to some further discoveries, relative to the production of the air yielded by water under other circumstances: and though my success in these inquiries has not been equal to my wishes, yet, as in the course of my researches I have discovered some facts which I take to be new, and as I have confirmed others, already known, by a variety of new experiments,

ments, I flatter myself that you will not think an account of my labours upon this subject altogether uninteresting.

Before I enter upon the detail of my experiments it will be necessary to premise, that I shall in general confine myself merely to the facts as they appear, without applying them to the confirmation or refutation of the theories of others, and without entering into any speculative enquiries relative to their remote causes; and in describing the different appearances I shall make use of the most familiar terms. Thus, in speaking of the air produced upon exposing raw silk in water to the action of light, I shall sometimes mention it as being yielded by the silk; and I shall sometimes speak of the air furnished by exposing water, which has previously turned green, in the sun's rays, as being immediately produced by the water, though it is probable, that the *green matter* acts a very important part in the production of this air in the one case and in the other. But how it acts is not well ascertained; and I had in general much rather confine myself to a simple, and even an unlearned, description of facts, than by endeavouring to give more precise definitions, at first, to involve myself in all the difficulties which would attend accounting for phænomena, whose causes are but very imperfectly known.

You will, therefore, not be surprised, if you should sometimes find me speaking of appearances in the same manner as a person would mention them who saw them for the first time, and who did not know that others had discovered them before, and how they had endeavoured to account for them. I shall take care that the facts shall be faithfully described, and I flatter myself you will not think them the less interesting on account of their being unadorned.—But I hasten to give you an account of my experiments.

Expe-

Experiment N° 1.

My first object was to collect a sufficient quantity of the air separated from water by silk to determine its goodness by the test of nitrous air; and to this end, having filled with clear spring water a globe of thin, white, and very transparent glass, $4\frac{1}{2}$ inches in diameter, with a cylindrical neck $\frac{1}{4}$ of an inch in diameter, and about 12 inches long, I introduced into it 30 grains of raw silk, which had been previously washed in water, in order to free it of air; and inverting the globe under water, and placing its neck in a glass jar, containing a quantity of the same water with which the globe was filled, I exposed it in my window to the action of the sun's rays, and prepared myself to examine the progress of the generation or production of the air.

The globe had not been exposed ten minutes to the action of the sun's rays, when I discovered an infinite number of exceedingly small air-bubbles, which began to make their appearance upon the surface of the silk; and these bubbles continuing to increase in number, and in size, at the end of about two hours the silk, appearing to be intirely covered with them, rose to the upper part of the globe.

These bubbles going on to increase in size, and running into each other, at length began to detach themselves from the silk, and to form a collection of air at the upper part of the globe; but the measure of my eudiometer being rather large, it was not till after the globe had been exposed in the sun near four days, that a sufficient quantity of air was collected to make the experiment with nitrous air, in order to ascertain its goodness by that test.

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Having

Having at length collected a sufficient quantity of this air for that purpose, I carefully removed it from the globe, and mixing with 1 measure of it 3 measures of nitrous air, they were reduced to 1,24 measures; which shews, that it was actually *dephlogisticated air*, and that of a considerable degree of purity.

Common air, tried at the same time, 1 measure of it with 1 measure of nitrous air were reduced to 1,08 measure.

Having again exposed the globe with the same water and silk in my window, where the sun shone the greatest part of the day, at the end of three days I had collected $3\frac{1}{2}$ cubic inches of air, which, proved with nitrous air, gave $1a + 3n = 1,18$; that is to say, 1 measure of this air, *added to* 3 measures of nitrous air, were reduced to 1,18 measure.

A small wax taper, which had been just blown out, a small part only of the wick remaining *red-hot*, upon being plunged into a phial filled with this air, immediately took fire, and burnt with a very bright and enlarged flame.

The water in the globe appeared to have lost something of its transparency, and had changed its colour to a very faint greenish cast, having at the same time acquired the odour or fragrance proper to raw silk.

This experiment I repeated several times with fresh water (retaining the same silk) and always with nearly the same result; with this difference, however, that when the sun shone very bright, the quantity of air produced was not only greater, but its quality likewise was much superior to that yielded when the sun's rays were more feeble, or when they were frequently intercepted by flying clouds. The air, however, was always not only much better than common air, but better than the air in general produced by the fresh leaves of plants exposed in water to the sun's rays in the experiments of Dr. INGEN-

HOUSZ;

HOUSZ; and under the circumstances the most favourable, it was so good that 1 measure of it required 4 measures of nitrous air to saturate it, and 3,65 measures of the two airs were destroyed; or, proved with nitrous air it gave $1a + 4n = 1,35$, which, I believe, is better than any air that has yet been produced in the experiments with vegetables.

The method I have here adopted of using algebraic characters in noting the result of the experiments made to determine the goodness of air, though not strictly mathematical, is very convenient; and for that reason, I shall continue to make use of it. a represents the air which is proved; n nitrous air; and the numbers which are joined to these letters shew the quantities, or the number of measures, of the different airs made use of in the experiment. The other number, which stands alone, or without any letter attached to it, on the other side of the equation, shows the volume, or the number of measures and parts of a measure to which the two airs are reduced after they are mixed. I shall sometimes add a fourth number, shewing the quantity of the two airs destroyed, as this more immediately shews the goodness of the air which is proved.

Thus, in the experiment last mentioned, 1 measure of the air proved, mixed with 4 measures of nitrous air, were reduced to 1,35 measure, consequently 3,65 measures of the two airs were destroyed; for it is $1 + 4 = 5 - 1,35 = 3,65$, and the result of this trial I should write thus, $1a + 4n = 1,35$, or 3,65.

Or, for still greater convenience in practice, as this last number 3,65, or $3\frac{65}{100}$ shews more immediately the goodness of the air in question, as I have just observed, by supposing with Dr. INGEN-HOUSZ the measure of the eudiometer to be divided into 100 equal parts, it will be $100a + 400n = 135$, and 365, expressing

expressing the volume of the two airs destroyed, will become a whole number.

But, instead of writing $100a + 400n = 135$, &c. I shall continue to write $1a + 4n = 1,35$, and shall express the last number (3,65) as a whole number notwithstanding; and I shall sometimes (following the example of Dr. INGEN-HOUZE) write this number *only*, in noting the goodness of any air in question.

I would just observe, with respect to the process of proving the goodness of any kind of air, by the test of nitrous air, that I mix the two airs in a phial, about 1 inch in diameter and 4 inches long, putting the air to be proved into the phial first, and then introducing the nitrous air, one measure after another, till the volume of the two airs after the diminution has taken place, amounts to more than *one* measure, and is less than *two* measures.

Immediately after the introduction of each measure of nitrous air, I give the phial a couple of shakes; after which I suffer it to stand at rest, while I prepare another measure of nitrous air, which commonly takes up about 20 seconds.

The measure of the eudiometer being filled with air, I suffer it to remain quiet under water 15 seconds, or while I can leisurely count 30, in order that the air may have time to acquire the temperature of the water in the trough, and that the water in the measure may have time to run down from the sides of the glass tube; and in shutting the slider I take care to bring it to be exactly even with the surface of the water in the trough. Similar precautions are likewise made use of in measuring the volume of the two airs in the tube of the eudiometer, after they have been mixed and diminished in the phial.

In order that I may know when I have added nitrous air enough to the air in the phial, so that the volume of the two

airs may amount to 1 measure, and may not be greater than 2 measures, there are two marks upon the phial, made with the point of a diamond, the one shewing 1 measure of my eudiometer, the other shewing 2 measures.

The tube of my eudiometer is half an inch in diameter internally, and 1 measure occupies $3\frac{1}{4}$ inches in length upon it, and the measure itself is made of a piece of the same tube. Both the one and the other are ground with fine emery on the inside, in order to take off the polish of the glass, and by that means facilitate the running down of the water, which might otherwise hang in drops upon the inside of the tube upon the introduction of air.

The nitrous air was always fresh made, and of the same materials, *viz.* fine copper wire dissolved in smoking spirits of nitre, diluted with 5 times its volume of water; and all possible attention was paid to every other circumstance that could contribute to the accuracy of the experiments.

I have thought it necessary to mention these particulars on account of the great difference in the apparent goodness of any kind of air proved by the test of nitrous air, which arises from the difference of the circumstances under which the experiments are made.

But to return to my experiments upon the air produced upon exposing silk in water to the action of the sun's rays.

Experiment N^o 2.

Finding that the quantity and the quality of the air produced depended, in a great measure, upon the intensity of the light by which the water and the silk were illuminated, I was desirous of seeing whether by depriving them intirely of all light, they would not at the same time be deprived of the power of
furnishing

furnishing air. To ascertain this fact, I took a globe A, similar to that made use of in the foregoing experiment, and having filled it with fresh spring water, I introduced into it 30 grains of raw silk, and placing it with its cylindrical neck inverted in a jar filled with the same water, I covered the whole with a large inverted earthen vessel, and exposed it, so covered up, for several days in my window, by the side of another globe B, containing a like quantity of water and silk, which I left naked, and consequently exposed to the direct rays of the sun.

The result of this experiment was, that the water and silk in the globe exposed to the sun's rays furnished air in great abundance, as in the experiment before-mentioned; while that in the globe covered up in darkness, produced only a few very inconsiderable air-bubbles, which remained attached to the silk.

Experiment N^o 3.

To see if heat would not facilitate the production of air in the globe sheltered from the light, I now removed it from the window, and placed it near a German stove, where I kept it warmed to about 90° of FAHRENHEIT's thermometer for more than 24 hours; but this was all to no purpose. The air produced was so exceedingly small in quantity, that it could neither be proved, nor measured, there being only a few detached air-bubbles, which had collected themselves near the top of the globe.

The medium heat of the water in the globe exposed in the sun's rays, at the time when it furnished air in the greatest abundance, was about 90° FAHRENHEIT. It was sometimes as high as 96°; but air was frequently produced in considerable quantities when the heat did not exceed 65° and 70°.

Experiment N^o 4.

Finding by the last experiment (N^o 3.) that heat alone, without light, was not sufficient to enable silk in water to produce air, I was desirous of seeing the effect of light, without heat, upon them. To this end, I took the globe B, with its contents, and plunging it into a mixture of ice and water brought it to the temperature of about 50° F. and taking it out of this mixture, and exposing it immediately in the sun's rays (which were very piercing at the time) I entertained it in this temperature above two hours by the occasional application of cloths, wet in ice water, to the lower part of the globe.

Notwithstanding this degree of cold, a considerable quantity of air was produced; though it was not furnished in so great abundance as when the globe was suffered to become hot in the sun's rays.

Having thus ascertained the great effect of the sun's rays in the production of the air furnished upon exposing silk in water to their influence, my next attempt was to determine, whether this arose from any peculiar quality in the sun's light; or whether *other light* would not produce the same effect. With a view to ascertaining this point, which I conceived to be of very great importance, I made the following interesting experiment.

Experiment N^o 5.

Having removed all the air from the globe B, and having supplied its place with a quantity of fresh water, so as to render it quite full, I replaced it inverted in its jar, and removing it into a dark room, surrounded it with 6 lamps with reflectors,
and

and 6 wax-candles, placed at different distances from 3 to 6 inches from it, and so disposed as to throw the greatest quantity of light possible upon the silk in the water, taking care at the same time that the water should not acquire a greater heat than that of about 90° F.

Things had not remained in this situation above 10 minutes, when I plainly discovered the air-bubbles beginning to make their appearance upon the surface of the silk; and at the end of 6 hours, there was collected at the upper part of the globe a quantity of air sufficient to make a proof of its goodness with nitrous air; and, upon trial, I had the pleasure to find, that it was *dephlogisticated*, and of such a degree of purity, that 1 measure of it with 3 measures of nitrous air occupied no more than 1,68 measure.

I afterwards exposed, to the same light, in small inverted glass jars, filled with water, a fresh-gathered healthy leaf of the peach tree, and a stem of the pea plant with 3 leaves upon it; and both these vegetables furnished air in the same manner as they are known to furnish it when exposed, under similar circumstances, to the action of the sun's direct rays, but in less quantities, which I attribute to the greater intensity of the sun's light above that of my lamps.

The experiment with the silk and water I repeated several times, always with nearly the same result. The quantity of air furnished was sometimes a little greater, and sometimes a little less; but it was always in much greater abundance than that furnished by an equal quantity of water and silk exposed to the same heat, but excluded from the light; and I have reason to think, it was of a much superior quality, though the quantity of that produced in the dark was too small to be submitted to any proof.

These experiments appear to me to be of so much importance, that I could wish they might be repeated, and varied, in such a manner as thoroughly to establish the facts relative to the subject in question. For my part, I would most readily undertake the investigation of the matter; but being employed in another pursuit (the continuation of my Experiments upon Heat), and, besides this, much of my time being taken up by the duties of my military employment, I have not leisure, at present, for such an undertaking.

Perhaps it may be proved by future experiment, that the matter of light is a constituent part of what is called pure or dephlogisticated air; if so, may we not venture to conclude with M. SCHÉELE, that the *light*, as well as the heat, produced by flame, and in general all burning bodies, arises *solely* from the decomposition of this air, and not from the phlogiston or inflammable principle of the body which is burnt? There are many phænomena which would seem to justify this opinion.

But to proceed in the account of my experiments.—The operation of inverting the globes under water, and placing them in the jars, and of displacing and replacing them upon removing the air produced, being attended with some inconveniences, I had recourse to another method of disposing of the apparatus, much more simple and more convenient. The globes being filled were laid upon a small piece of deal board, with their necks inclined at an angle of about 20° above the plane of the horizon, and supported in this position by a perpendicular fork of wood, fixed to the end of the board, as represented by the figure. (See Tab. VI.) The part of the board, upon which the under part of the globe reposed, was hollowed a little, to prevent the globe from rolling; or, what I found
more

more safe and convenient, a small ring, or hoop, of soft wood, was nailed down upon the board in that part.

By this arrangement the jars were spared, and the end of the neck of the globe being easy to be come at, by introducing a wire, or, what I commonly made use of in preference, a small glass tube, into the globe, the air hanging attached to the silk can at all times be separated from it; which is often necessary, in order to determine with greater precision the quantity of air furnished in any given time.

The air produced naturally rises to that part of the globe which is uppermost, where it collects in a body, driving out an equal volume of water; which, to prevent its running about, may be collected, by placing a proper vessel under the mouth, or end of the neck of the globe, to receive it.

The method of removing the air from the globe is too well known to require a description. I would however observe, that in doing it care should be taken, that the water in which the globe is immersed be quite clean, and of the same kind with that in the globe, otherwise that which enters the globe, to replace the air removed, might derange the experiment.

Having provided myself with a number of globes of different sizes, all fitted with boards or stands to support them, in the manner above described, I proceeded in the course of my experiments as follows.

Finding that raw silk, exposed in water to the action of light, causes the water to yield pure air in so great abundance, I was desirous of finding out whether this arose from any peculiar quality possessed by the silk; or whether other bodies might not be made to produce the same effect: to this end, having provided 6 globes, each about $4\frac{1}{2}$ inches in diameter, and having filled them with fresh spring-water, I introduced into them.

them the following substances, and exposed them all, at the same time, to the action of the sun's rays.

In the globe N° 1. I put 15 grains of sheep's wool,

N° 2. — 15 grains of Eider down,

N° 3. — 15 grains of the fine fur of a Russian hare,

N° 4. — 15 grains of cotton wool,

N° 5. — 15 grains of lint, or the ravelings of fine linen,

N° 6. — 15 grains of human hair; these substances being all well washed, and being thoroughly freed of air, by being wet before they were put into the globes.

The results of these experiments were as follows.

Experiment N° 6.

The globe N° 1. which contained the sheep's wool, did not begin to furnish air in any considerable quantity till the third day of its being exposed to the action of the sun's rays; and several days of cloudy weather intervening, I did not remove the air till the eighth day, when I collected $1\frac{1}{2}$ cubic inch, which, proved with nitrous air, gave $1a + 3n = 1,28$, or 272 degrees.

The wool at no time furnished more than one-third part of the air, which an equal quantity of silk would have furnished under the same circumstances.

The water was very faintly tinged of a greenish hue.

Experiment N° 7.

The water in the globe N° 2. with the Eider down, began almost immediately to furnish air, and continued to yield it during the whole time of the experiment, nearly in as large quantities

quantities as the water with silk had done in the former experiments, and nearly of the same quality. $1\frac{1}{2}$ cubic inches of this air, furnished the eighth day from the beginning of the experiment, or the third of sunshine, proved with nitrous air gave $1a + 3n = 1,34$, or 266 degrees of purity.

The water was faintly tinged of a greenish, yellowish cast, and the Eider down, when examined attentively, appeared to be covered with a greenish slime.

Experiment N° 8.

The globe N° 3. with the hare's fur (which was white) furnished more air than the sheep's wool, but not so much as the Eider down. After four days of sunshine, I collected 2 cubic inches of this air, which, proved with nitrous air, gave $1a + 3n = 1,44$, or 256.

The water had acquired a very faint yellowish hue; but it did not appear to have lost much of its transparency, or to be disposed to deposit any sediment.

The air produced in this experiment made its appearance in a different manner from that furnished in those preceding it, the air-bubbles which appeared upon the surface of the fur being at considerable distances from each other, and growing to an uncommon size before they detached themselves to rise to the surface of the water.

Experiment N° 9.

The globe N° 4. with cotton wool furnished a considerable quantity of air which appeared to be better than that furnished by any of the five other globes. Proved with nitrous air, it turned out $1a + 3n = 1,07$, or 293; and, what was particular, the water did not appear to have altered its colour in the least, or to have lost any thing of its transparency.

Experiment N° 10.

The globe N° 5. with ravelings of linen, was very tardy in furnishing air, and produced but a small quantity; at the end of a fortnight, however, I collected about 2 cubic inches, which, proved with nitrous air, gave $1a + 3n = 1,51$, or 249.

The air appeared to have very little disposition to fix itself to the surface of this substance. It was very seldom that there were air-bubbles enough attracted to it to cause it to rise to the surface of the water, and the few bubbles which occasionally made their appearance very soon disappeared upon the diminution of the light and heat of the sun. In short, it appeared, that there is but a very feeble attraction between this substance and the particles of air, at least when they are dissolved in water. Whether this arises from the superior affinity of the substance to water, or not, I will not pretend to decide; but it appears to be probable, as there is so strong an attraction between water and linen, or flax, which is apparent from the avidity with which a piece of dry linen drinks up that fluid, and becomes wet, even to a considerable distance, when one end of it only is placed in it.

You will recollect that I here consider the separation of the air from water as a simple operation; and that I do not take into the account the purification, or rather the generation, of this air. Though there is great reason to conclude, that these two operations are very nearly connected; yet, to simplify my inquiries, I shall, in the first place, consider the appearances as they presented themselves to my senses. It will be easy afterwards to draw any conclusions from the results of the experiments, which a careful examination and comparison of the various phænomena will justify.

Expe-

Experiment N° 11.

The globe N° 6. with human hair, furnished still less air than that with ravelings of linen in the last mentioned experiment; but, notwithstanding the smallness of the quantity, it was considerably superior in quality to atmospheric air, for, proved with nitrous air, it gave $1a + 2n = 1,45$, or 155; whereas common air, proved at the time, gave $1a + 1n = 1,08$, or 92.

Experiment N° 12.

To ascertain the relative goodness of the air furnished by the water in these experiments, and of that produced by exposing fresh healthy vegetables in water to the action of the sun's light, according to the method of Dr. INGEN-HOUSZ, I collected a small quantity of air from a stem of a pea plant, which had four healthy leaves upon it, and found it to be much inferior to that furnished in the experiments with silk, and the various other substances I made use of. Proved with nitrous air, it gave $1a + 2n = 1,05$, or 195.

An intire plant of housewort, of a moderate size, exposed in 12 ounces of water 7 hours, to the action of the sun's rays, at a time when the weather was remarkably fine, and very hot, furnished about $\frac{1}{4}$ of a cubic inch of air, which was so much worse than common air, that 1 measure of it, with 1 measure of nitrous air, occupied 1,36 measures; or it was $1a + 1n = 1,36$, or 64. But I lay no kind of stress upon the result of this experiment, as it is more than probable, that the badness of the air arose from the roots of the plants; for from the leaves alone I have frequently since obtained air, which appeared to be considerably better than common air.

From the leaves of the peach-tree I obtained an air which, proved with nitrous air, gave $1a + 2n = 1,32$, or 168; but I did not think it necessary to multiply these experiments, particularly as Dr. INGEN-HOUSZ and Mr. SENNEBIER have given us the results of so many of theirs upon the same subject, of the accuracy of which there is no room left to doubt. I shall therefore content myself with referring to the results of their experiments.

With a view to determining, with greater precision, the quantity and the quality of the air produced by a given quantity of water and silk, exposed for a given time to the action of the sun's rays, I made the following experiment.

Experiment N° 13.

A globe of fine, clear, white glass, about $8\frac{1}{10}$ inches in diameter, and containing 296 cubic inches, being filled with fresh spring water and 30 grains of raw silk, was exposed in my window three days, viz. the 12th, 13th, and 14th of May last, these days being for the most part cold and cloudy, with short intervals of sunshine. Air produced $9\frac{1}{2}$ cubic inches; quality $1a + 3n = 1,61$, or 239.

May 15. This air being removed, and its place supplied with fresh water, the globe exposed in the sun this day from nine o'clock in the morning till five o'clock in the afternoon, the weather being very fine, yielded $8\frac{4}{10}$ cubic inches of air, which, proved with nitrous air, gave $1a + 4n = 1,74$, or 326. The heat of the water in the globe, during the experiment, was from 70° to 98° F. The water had now lost considerably of its transparency, and had assumed a light greenish hue.

May 16. The air furnished yesterday being removed, the globe furnished this day, during six hours of sunshine, 9 cubic inches

inches of air, which, proved with nitrous air, gave $1a + 4n = 1,44$, or 356.

May 17. The globe furnished this day, during $3\frac{1}{2}$ hours of sunshine, 6 cubic inches of air, of a very eminent quality; for, proved with nitrous air, it gave $1a + 4n = 1,35$, or 365.

May 18. This day cold and cloudy; not more than $1\frac{1}{2}$ hours sunshine; air produced $\frac{1}{4}$ of a cubic inch; quality $1a + 4n = 1,56$, or 344.

May 19. The globe appearing now to be quite exhausted of air, shewing no signs of furnishing any additional quantity, though exposed to the action of a very bright sun, I removed the globe from the window, and placed it by the side of a German stove, where it was kept warm to 100° F. from 10 o'clock in the morning till 5 o'clock in the afternoon. By this means I obtained $\frac{1}{4}$ of a cubic inch of air, which, proved with nitrous air, gave $1a + 4n = 1,74$, or 326.

Not being able to obtain any more air from the globe, I now put an end to the experiment.

The quantities and qualities of the airs furnished upon the different days were as follows :

	Quantity.	Quality.
Upon the 12th, 13th, and 14th of May	$9\frac{1}{2}$ cubic inches	$1a + 3n = 1,61$, or 239
15th	$8\frac{1}{8}$	$1a + 4n = 1,74$, or 326
16th	9	$1a + 4n = 1,44$, or 356
17th	6	$1a + 4n = 1,35$, or 365
18th	$\frac{1}{4}$	$1a + 4n = 1,56$, or 344
19th	$\frac{1}{4}$	$1a + 4n = 1,74$, or 326
Total quantity	$33\frac{9}{8}$	Mean quality $1a + 4n = 1,84$, or 326

As in this experiment the air furnished each day was removed at night, and the place it occupied in the globe supplied with fresh

fresh water, I was desirous of seeing what variation it would occasion in the result of the experiment, if, instead of removing the air from time to time, I suffered it to remain in the globe till the end of the experiment: to this end I made

Experiment N° 14.

In which the globe being filled with fresh water, and the silk used in the last experiment (being first well washed), the whole was exposed four days to the action of the sun's rays, the weather being remarkably fine, and very hot. Upon removing the air produced, I found it amounted to $30\frac{1}{8}$ cubic inches; and its quality, proved with nitrous air, was $1a + 3n = 1,02$, or 298,

I should have continued the experiment for some days longer, as the globe did not appear to be exhausted; but the quantity of air already collected in the globe was so great that it became very difficult to remove it, without running the risk of losing a part of it, or of letting the air of the atmosphere enter the globe, either of which would of course have spoiled the experiment. For safety therefore, and that I might not by an accident lose the trouble I had already had with it, I put an end to the experiment at the end of the fourth day.

The water had lost of its transparency, and had acquired a greenish cast, as in the last experiment; and in both these experiments I observed, that a considerable quantity of whitish yellowish earth was precipitated by the water, which, falling to the bottom of the globe, attached itself to the glass in such a manner that it was with difficulty that it could be removed. These were general appearances, and took place in all cases, in a greater or less degree, where a considerable quantity of pure air was separated from water by the influence of light.

Experiment N^o 15.

The filk made use of in the last experiment having been frequently used in the foregoing experiments, I was desirous of seeing the effect of making use of fresh filk; and also of varying the proportion between the quantity of filk, the quantity of water, and the size of the globe; accordingly, at 6 o'clock P.M. upon the 13th of June, I filled a small globe, about 3 inches in diameter, or (to ascertain its size more exactly) which contained just 20 cubic inches, with fresh spring water, and 17 grains of raw filk, wound in a single thread, which had never been put into water, or otherwise used, since it came out of the hands of the filk-winder.

At the end of four days, *viz.* the 14th, 15th, 16th, and 17th of June, this globe had only furnished $\frac{1}{4}$ of a cubic inch of air, which, proved with nitrous air, gave $1a + 1n = 1,32$, or 68; consequently was much worse than common air.

Upon the 18th, it began to produce good air, and during six hours of sunshine it furnished $1\frac{1}{10}$ cubic inches, which, proved with nitrous air, gave $1a + 3n = 1,15$, or 285.

The two following days (*viz.* the 19th and 20th of June) it furnished $1\frac{2}{10}$ cubic inch of air, which, proved with nitrous air, gave $1a + 3n = 1,37$, or 263; after which it totally ceased to yield air, though exposed for several days in the sun's rays.

Total quantity of air produced $2\frac{6}{10}$ cubic inches; mean quality $1a + 3n = 1,46$, or 254.

By this experiment it appears, that raw filk, when used for the first time, does not immediately dispose the water to yield pure air; on the contrary, that it phlogisticates the air yielded by

by water to a very considerable degree; and this I afterwards found to be the case with several other substances.

Though the quality, at a medium, of the air furnished in this experiment was not quite so good as that furnished in the two experiments last mentioned (*viz.* N° 13. and N° 14.), yet its quantity, in proportion to the quantity of water made use of, was greater than in either of them: it amounted to something more than *one-eighth* of the volume of the water.

Of all the substances I had hitherto made use of in these experiments, raw silk had furnished the greatest quantity of pure air; or, to express myself more properly, had caused the water to furnish the greatest quantity; but it appeared to me very probable, that some other body might be found, that possessed this property in a still greater degree than silk. Turning this matter in my mind, it occurred to me, to make the experiment with the silky, or rather cotton-like, substance produced by a certain species of the Poplar-tree, *Populus nigra*, very common in this country, and which, I believe, grows in England. I recollected that examining it some time before, with a different view (that of seeing if it might not be made use of with advantage, as a substitute for Eider down), and endeavouring to render it very dry, by exposing it in a china plate over a chafing-dish of hot embers, when it had acquired a certain degree of heat small parcels of it quitted the plate of their own accord, and mounted up to the top of the room.

This convinced me of the time not only of its extreme fineness, but also of the strong attraction which subsists between it and the particles of air; and it now occurred to me, that these qualities not only render it peculiarly proper as a substitute of Eider down, for confining heat, but likewise are properties

properties of all others the most necessary to its supplying the place of silk in the production of air, by exposing it in water to the action of the sun's rays. I therefore lost no time in making the following experiments.

Experiment N° 16.

The great globe (contents 296 cubic inches) being filled with fresh spring water, and 120 grains of poplar cotton, upon the evening of the 9th of June, and being the next day, the 10th of June, exposed to the sun about four hours, upon the morning of the 11th the air produced was removed, and its quantity was found to be $1\frac{1}{4}$ cubic inch. Its quality was very bad, *viz.* $1a + 1n = 1,65$, or 35 degrees only better than thoroughly phlogisticated air.

Upon the 11th, 12th, and 13th, 1 cubic inch of air only was produced, and this appeared to be as bad as possible; for, proved with nitrous air, it gave $1a + 1n = 2$, or 0.

Upon the 14th a few air-bubbles only were furnished; but, notwithstanding these unfavourable appearances, I still continued the experiment, and my patience was amply rewarded; for the next day, the 15th, the sun being very powerful, and the weather very hot, the water changing suddenly to a greenish colour, began all at once to give good air in great abundance. In the course of the day $10\frac{1}{1000}$ cubic inches were produced, which, proved with nitrous air, gave $1a + 3n = 1,43$, or 257.

June 16th, a very warm clear day. The globe exposed in the sun, from 8 o'clock in the morning till 5 o'clock in the afternoon, furnished $14\frac{1}{1000}$ cubic inches of air, which, proved with nitrous air, gave $1a + 3n = 1,34$, or 266.

June 17th, cloudy, with intervals of sunshine. The globe with about 4 hours sun gave $7\frac{1}{10}\frac{4}{10}$ cubic inches of air, of a very eminent quality, viz. $1a + 4n = 1,40$, or 360.

The water having by degrees lost its transparency, and having acquired a deep green colour, it broke up this day, and deposited a green sediment; after which it recovered its transparency, and became almost colourless. It continued, notwithstanding, to furnish air in considerable quantities.

June 18th, being exposed in the sun's rays from 8 o'clock in the morning till 2 o'clock in the afternoon (when the heavens became overcast), the globe yielded $6\frac{1}{10}\frac{7}{10}$ cubic inches of air, which, proved with nitrous air, gave $1a + 4n = 1,44$, or 356.

June the 19th and 20th. These two days the globe furnished no more than $3\frac{1}{10}\frac{3}{10}$ cubic inches of air, which, proved with nitrous air, gave $1a + 3n = 1,06$, or 294; after which it ceased totally to furnish air, and the colour of the water changed to a dead yellowish cast, and the cotton assumed the same hue.

The following are the quantities and qualities of the different parcels of air furnished in the course of this experiment.

	Quantity.	Quality.
Upon the 10th of June	$1\frac{1}{2}$ cubic inches	$1a + 1n = 1,65$, or 35
11th, 12th, and 13th	1	$1a + 1n = 2$, or 0
14th	0	—
15th	$10\frac{4}{10}\frac{2}{10}$	$1a + 3n = 1,43$, or 257
16th	$14\frac{3}{10}\frac{4}{10}$	$1a + 3n = 1,34$, or 266
17th	$7\frac{3}{10}\frac{4}{10}$	$1a + 4n = 1,40$, or 360
18th	$6\frac{2}{10}\frac{7}{10}$	$1a + 4n = 1,44$, or 356
19th and 20th	$3\frac{1}{10}\frac{3}{10}$	$1a + 3n = 1,06$, or 294
Total quantity	$44\frac{1}{2}$	Mean quality $1a + 3n = 1,23$, or 277

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This experiment was repeated, and with nearly the same result; the total quantity of air produced being $41\frac{1}{2}$ cubic inches, and its quality, at a medium, $1a + 3n = 1,26$, or 274.

To ascertain the relative fineness of this poplar cotton, and the thread of raw silk as spun by the worm, in order to make an estimate of the surface of the former, I examined them both at the same time under an excellent microscope, when the diameter of the cotton, that is to say, of a single thread or fibre of it, appeared to be not more than half as great as the diameter of the silk, consequently its diameter was not more than $\frac{1}{1.5}$ part of an inch; for I have shewn, in a former letter, that the diameter of a thread of silk, as spun by the worm, is only $\frac{1}{1.5}$ of an inch.

The specific gravity of the cotton I found to be very nearly equal to that of water, consequently it is to that of silk as 1000 to 1734; its surface, therefore, is to the surface of an equal weight of raw silk in the compound proportion of 2 to 1, and of 1734 to 1000; that is to say, as 3468 to 1000.

Now, as the surface of 30 grains of raw silk amounts to 476 square inches, the surface of 30 grains of poplar cotton must amount to 1651 square inches, which gives 55 square inches of surface for each grain in weight; consequently the surface of the cotton made use of in the foregoing experiment (N^o 16.) did not amount to less than 6600 square inches (for 120 grains, the weight of the cotton, multiplied by 55, gives 6600); an enormous surface indeed for a body, whose *solid contents* did not amount to quite half a cubic inch.

From hence it appears evidently, that the quantities of air furnished by water, in the experiments with raw silk, and with poplar cotton, were neither in proportion to the quantities of these substances made use of, nor to the quantities of their

surfaces. It appears likewise, from the two last experiments, that the air which is furnished in the beginning of the experiment, or when the water is first exposed to the action of the sun's rays, is neither so good, nor in so great abundance, as afterwards, at a more advanced period; and that it totally ceases to be produced after a certain time.

To ascertain, with greater precision, the qualities of the air furnished at different periods of the experiment, or rather the period when the water begins to give good air; and also to determine the relative quantities and qualities of the airs produced in the experiments with raw silk, and in those with poplar cotton, I made the following experiments.

Experiment N° 17.

A globe, about $4\frac{1}{2}$ inches in diameter, containing just 46 cubic inches, being filled in the evening with fresh spring water, and 30 grains of raw silk which had been previously washed thoroughly to free it of air and the remains of former experiments, and being exposed the next day in my window, the weather being cold and cloudy, with not more than 1 hour of sunshine, $\frac{1}{4}$ of a cubic inch of air was produced, which, proved with nitrous air, gave $1a + 2n = 1,86$, or 114.

The two following days, the weather being clear and moderately warm, $3\frac{1}{4}$ cubic inches of air were produced, which, proved with nitrous air, gave $1a + 3n = 1,14$, or 296.

Experiment N° 18.

The globe having been again filled with fresh spring water, and the same silk which had served in the last experiment, after 2 nights, and 1 day of about 4 hours sun, it had furnished $1\frac{1}{4}$ cubic inch of air, whose quality was $1a + 2n = 1,13$, or 197.

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The two following days, the weather being very fine, it furnished $3\frac{1}{5}$ cubic inches of air, which, proved with nitrous air, gave $1a + 4n = 1,58$, or 342.

Experiment N° 19.

The globe being again filled with fresh water, and the same silk well washed, and being exposed 2 days in the sun, it gave $2\frac{1}{5}$ cubic inches of air, which, proved with nitrous air, gave $1a + 3n = 1,67$, or 233.

Experiment N° 20.

A like globe, with fresh water, and an equal quantity of poplar cotton which had been used in former experiments, being exposed at the same time, gave $2\frac{1}{5}$ cubic inches of air, whose quality was $1a + 3n = 1,20$, or 280.

Experiment N° 21.

A small globe, contents 20 cubic inches, with 17 grains of raw silk, exposed at the same time, gave 1 cubic inch of air, which turned out $1a + 3n = 1,37$, or 263.

Experiment N° 22.

A large globe, containing 296 cubic inches, being filled with fresh water and a small quantity of *conserva rivalaris*, and exposed at the same time with the three globes above-mentioned, gave $1\frac{1}{2}$ cubic inch of air, which, proved with nitrous air, gave $1a + 2n = 1,76$, or 124.

The water in this experiment was changed to a brown colour, owing, as I conceived, to the too great heat the *conserva* acquired in the sun.

These experiments were made between the 2d and the 5th of July.

Expe-

Experiment N° 23.

Surprised at the smallness of the quantity, and the inferior quality, of the air produced in the last experiment, I was induced to repeat it; accordingly, the globe being again filled with water, and a quantity of fresh *conserva rivularis* (a small handful), and being exposed to the action of the sun's rays during 3 fine days, $13\frac{3}{8}$ cubic inches of air were produced, which, proved with nitrous air, gave $1a + 3n = 1,54$, or 246.

At the end of the experiment the water appeared to be very faintly tinged of a greenish cast.

The two following experiments were made upon the 20th and 21st of August.

Experiment N° 24.

A globe, about $4\frac{1}{2}$ inches in diameter (contents 46 cubic inches) being filled with fresh spring water, and 30 grains of raw silk which had been used in many preceding experiments, and being exposed to the action of the sun's rays two days, in all about 8 hours of sunshine, the weather being cloudy great part of the time, $17\frac{5}{8}$ cubic inch of air was produced, which, proved with nitrous air, gave $1a + 3n = 1,96$, or 294.

Experiment N° 25.

At the same time an equal globe, containing fresh spring water, and about 15 grains of poplar cotton (which had likewise been used in former experiments) produced $17\frac{5}{8}$ cubic inch of air, which, proved with nitrous air, gave $1a + 3n = 1,49$, or 260.

The

The water in both these experiments had acquired a faint greenish cast; but the colour of that with the cotton was rather the deepest.

Upon examining this water under a microscope, I found it contained a great number of animalcules, exceedingly small, and of nearly a round figure. That with the silk contained the same kind of animalcules likewise, but not in so great abundance. I never failed to find them in every case in which the water used in an experiment had acquired a greenish hue; and from their presence alone, I think it more than probable, that the colour of the water, *in the first instance*, arose in all cases. I have spent a great deal of time in observing them, and have made many experiments upon their production; but as I have not yet been able to satisfy my own mind, with respect to the part they act in the operation of purifying the air in water, I shall not add to the length of this letter by giving an account of my enquiries and observations respecting them.

I was yet by no means satisfied with respect to the part which the silk and other bodies, exposed in water in the foregoing experiments, acted in the purifying or dephlogisticating of the air produced.

Dr. PRIESTLEY has long since discovered, that many animal and vegetable substances putrefying, or rather dissolving, in water, in the sun, cause the water to yield large quantities of dephlogisticated air; but I could hardly conceive, that the small quantity of silk which was used in my experiments, and which had been constantly in water for more than three months, and had so often been washed, and even boiled in water, should yet retain a power of communicating any thing to the large quantities of fresh water in which it was successively placed; at least *any thing* in sufficient quantities to impregnate

nate those bodies of water, and to cause them to yield the great abundance of air which they produced.

It was still more difficult to account for the purification of the air in the experiments with wool and fur, and human hair; especially, as in some of these experiments the water had not sensibly changed colour, nor did it appear to have lost any thing of its transparency. It is true, in these cases, the quantities of air produced were very small; but yet its quality was better than that of common air, and considerably superior to that of the air existing in the water, previous to its being exposed to the action of the sun's light. In short, it was dephlogisticated in the experiment; but the *manner* in which this was done is very difficult to ascertain.

With a view to throwing some new light upon this intricate subject, I made the following experiments.

Experiment. N° 26.

Concluding that if silk and other bodies, used in the foregoing experiments, actually did not contribute any thing, considered as chymical substances, in the process of the production of pure air yielded by water; but if, on the contrary, they acted merely as a mechanical aid in the *separation* of the air from the water, by affording a convenient surface for the air to attach itself to; in this case, any other body, having a large surface, and attracting air in water, might be made use of instead of silk in the experiment, and pure air would be furnished, though the body so made use of should be totally incapable of communicating *any thing whatever* to the water.

To ascertain this fact, washing the great globe (containing 296 cubic inches) perfectly clean, and filling it with fresh spring water, I introduced into it a quantity of the fine flexible
thread

thread of glass, commonly called *spun glass*, such as is used for making brushes for cleaning jewels, and for making a kind of artificial feather frequently sold by the Jew pedlars. This spun glass is no other than common glass drawn out, when hot, into an exceeding fine thread; which thread, in consequence of its extreme fineness, retains its flexibility after it has grown cold.

I made choice of this substance not only on account of its great surface, but also on account of the strong attraction which is known to subsist between glass and air, and the impossibility of its communicating any thing to the water.

The result of the experiment was, that the globe being exposed in the sun, air-bubbles began almost immediately to make their appearance upon the surface of the spun glass, and in 4 hours $\frac{77}{100}$ of a cubic inch of air was collected, which, proved with nitrous air, gave $1a + 1n = 1,12$, or 88; after which, not a single air-bubble more was produced, though the globe was exposed a whole week in the window, during which time there were several very warm, fine, sunshiny days.

This experiment shews evidently, that something more is wanting to the production of pure air by water, exposed in the sun, than merely a surface to which the air dissolved in the water can attach itself, in order to its making its escape.

The air furnished in this experiment was doubtless merely that with which the water issuing from the earth was overcharged, and which would have made its escape from the water, had the water, instead of being exposed with the spun glass in the sun, been simply left for some time exposed to the free air of the atmosphere.

It appears, that this air, naturally existing in spring water, instead of being dephlogisticated, is something worse than

common air; and this agrees with the observations of Dr. PRIESTLEY, and seems to justify his opinion with respect to the cause of the fertility of lands washed by waters issuing from the earth.

If the above experiment shews that something is wanted to be mixed with water, in order to enable it to yield pure air, when exposed to the action of the sun's light, the following shew, that this *something*, whatever it may be, is frequently to be found in the water itself, in its natural state.

Experiment N° 27.

A large jar of clear white glass, containing 455 cubic inches, being washed very clean, was filled with fresh spring water, and inverted in a glass basin of the same, and placed in the middle of the garden of the Elector's Palace, where it was left exposed to the weather 28 days.

At the same time another like jar was filled with water, taken from a pond in the garden, in which many aquatic plants were growing, and was exposed in the same place, and during the same period. This water had a very faint greenish cast. The pond from which it was taken is fed by a large river (the Isar), which runs by the town.

The second day after these waters had been exposed in the sun, I observed, that a small quantity of air had collected itself at the upper part of each of the jars.

The third, fourth, and fifth days, the pond water furnished air in pretty large quantities; and it went on to yield it without intermission, when the sun shone upon it, till the fourteenth day, when it seemed to be nearly exhausted. I continued the experiment, however, till the twenty-eighth day, though during the

the last fortnight the quantity of air in the jar did not appear to be sensibly increased.

The spring water, during the first five or six days, furnished very little air; and it was not till the fourteenth day that it began to yield it in any considerable quantities. From this time it went on to furnish it, though but very slowly, till about the twenty-second day, when it ceased, appearing to be quite exhausted.

Upon the twenty-eighth day I removed the airs from the jars, when I found their quantities and qualities to be as follows:

	Quantity.	Quality.
Air furnished by the spring water	14 cubic inches	$1a + 27 = 1,51$, or 138
by the pond water	$31\frac{1}{2}$ —	$1a + 37 = 1,48$, or 252

Neither the colour of the spring water, nor that of the pond water, appeared to be sensibly changed; but both the one and the other of these waters had deposited a considerable quantity of earth, which was found adhering to the surfaces of the glass basons in which the jars were inverted.

As these basons were rather deep, and as they were very thick in glass, and consequently not very transparent, their bottoms, where the sediment of the water was collected, were, in a great measure, obscured or deprived of the direct rays of the sun. Suspecting that this circumstance might have had some effect, so as to have hindered the water from furnishing so much air as otherwise it might have yielded, to satisfy myself respecting this matter I repeated the experiment, disposing the apparatus in such a manner, that the sediment of the water, which attached itself to the bottom of the vessel in which the jar was inverted, had the advantage of being perfectly illuminated.

Experiment N° 28.

In a large cylindrical jar, of very fine transparent glass, 10 inches in diameter, and 12 inches high, filled with spring water, I inverted a conical glass jar, $9\frac{1}{4}$ inches in diameter at the bottom, and containing 344 cubic inches, filled with the same water; and exposed the whole 21 days, in a window fronting the south.

The quantity of air produced amounted to 40 cubic inches; and its quality, proved by the test of nitrous air, gave $1a + 3n = 1,87$, or $21\frac{1}{2}$.

The water in this experiment furnished very little air till the seventh day; but after that time, having assumed a faint greenish cast, and a fine greenish slimy sediment (the *green matter* of Dr. PRIESTLEY) beginning to be formed upon the bottom of the jar, it began to yield air in abundance, and continued to furnish it in pretty large quantities till about the eighteenth day, when it appeared to be exhausted.

Why the water should turn green in this experiment, and not in the last, I know not; unless it was in consequence of the large surface of water in the cylindrical jar, which was exposed to the air in this experiment; or in consequence of the sun's shining directly upon the bottom of the vessel where the sediment was formed.

In the former experiment the basin in which the jar was inverted was but just big enough to admit the jar; and as the jar was cylindrical, the surface of the water exposed to the atmosphere, in the basin, was but very small; and the basin being very thick, and formed of glass which, though of the white kind, was of an inferior quality, and very imperfectly transparent, as I have already observed, the bottom of the basin,

where the sediment was formed, was but very imperfectly illuminated.

I intended to have repeated these experiments with variations, and to have made several others which I had projected, and which I thought might have thrown some further light upon this wonderful process of the production of the pure air yielded by water; but a series of unfavourable weather putting a stop to my enquiries, and my time having been much taken up since with other avocations, I have hitherto been prevented from putting my designs in execution; and the season proper for these experiments is now so far advanced, that I do not think it will be in my power to recommence them till the next year. In the meantime, to fulfil my promise to you, I send you this account of the progress I have already made in these researches; and, when I shall find leisure to pursue the matter further, I shall not fail to acquaint you with the result of my enquiries.

I have the honour to be, &c.

P O S T S C R I P T.

SINCE writing the above, an interval of fine weather, and a moment of leisure, have given me an opportunity of making a few more experiments, of which I have thought it right to give you a short account.

And I must begin by acquainting you, that having never been thoroughly satisfied with respect to the origin of the dephlogisticated air produced upon exposing fresh vegetables in water to the action of the sun's rays, according to the method of Dr. INGEN-HOUSZ, my doubts, with respect to the opinion generally entertained of its being *elaborated* in the vessels of the plant,

plant, instead of being removed, were rather confirmed by the result of the experiments of which I have given an account in the foregoing letter; and however disposed I was to adopt the beautiful theory of the purification of the atmosphere by the vegetable kingdom, I was not willing to admit a fact which has been brought in support of it, till it should appear to me to have been demonstrated by the most decisive experiments.

That the fresh leaves of certain vegetables, exposed in water to the action of the sun's rays, cause a certain quantity of pure air to be produced, is a fact which has been put beyond all doubt; but it does not appear to me to be by any means so clearly proved, that this air is "*elaborated* in the plant by the "powers of vegetation;"—"phlogisticated or fixed air being "first absorbed or imbibed by the plant as food, and the dephlogisticated air being rejected as an excrement:" for, besides that many other substances, and in which no elaboration, or circulation, can possibly be suspected to take place, cause the water in which they are exposed to the action of light to yield dephlogisticated air as well as plants, and even in much greater quantities, and of a more eminent quality, the circumstances of the leaves of a vegetable, which, accustomed to grow in air, are separated from its stem, and confined in water, are so unnatural, that I cannot conceive, that they can perform the same functions in such different situations.

Among many facts which have been brought in support of the received opinion of the *elaboration* of the air in the vessels of the plants in the experiments in question, there is one upon which great stress has been laid, which, I think, requires further examination.

The fresh healthy leaves of vegetables, separated from the plant, and exposed in water to the action of the sun's rays,

appear, by all the experiments which have hitherto been made, to furnish air *only for a short time*; after a day or two, the leaves changing colour, cease to yield air: and this has been conceived to arise from the powers of vegetation being destroyed; or, in other words, the death of the plant; and from hence it has been inferred, with some degree of plausibility, not only that the leaves actually retained their vegetative powers for some time after they were separated from their stock, but that it was in consequence of the exertion of these powers, that the air, yielded in the experiment, was produced.

But I have found, that though the leaves, exposed in water to the action of light, actually do cease to furnish air, after a certain time, yet that they *regain* this power after a short interval, when they furnish (or rather cause the water to furnish) more and better air than at first, which can hardly be accounted for upon the supposition that the air is *elaborated* in the vessels of the plant.

Experiment N^o 29.

A globe, containing 46 cubic inches, filled with fresh spring water and two peach leaves, was exposed in the window to the action of the sun's rays, 10 days successively (the weather being in general fine), when the following appearances took place.

The 1st and 2d day, a certain quantity of air was produced, about as much as in former like experiments. The 3d day very little was produced; and the 4th day none at all, the globe to all appearance being quite exhausted. Continuing the experiment, however, upon the 5th day, the water having acquired a faint greenish hue, air was again produced pretty plentifully, *making its appearance upon the surface of the leaves in the form of air-bubbles.*

bubbles, as at the beginning of the experiment; at the end of the 6th day the air was removed, and it was found to amount to $\frac{1}{100}$ of a cubic inch, its quality being 232 degrees, or $1a + 3n = 1,68$.

Upon the 7th day $\frac{1}{100}$ of a cubic inch of air was produced of 297 degrees, or $1a + 3n = 1,03$; and

During the 8th, 9th, and 10th days, $1\frac{1}{2}$ cubic inch of air, of 307 degrees (or $1a + 4n = 1,93$), was furnished; after which an end was put to the experiment.

Total quantity of air produced $3\frac{1}{100}$ cubic inches; mean quality 291 degrees, or $1a + 3n = 1,09$.

Finding that leaves which were dead, or in which all the powers of vegetation were evidently destroyed, continued notwithstanding to separate air from water, and that in so great abundance, I was desirous of seeing the effect of exposing fresh healthy leaves in water which I knew to be previously saturated with, and disposed to yield dephlogisticated air. I conceived, that if the plants exposed in water actually imbibed fixed or phlogisticated air as food, and, after digesting it, "discharged" the dephlogisticated air as an excrement;" in that case, as there is no instance of any plant, or animal, being able to nourish itself with its own excrement, the leaves exposed in water saturated with dephlogisticated air, instead of imbibing and elaborating it, would immediately die.

The experiments which I made to ascertain this fact, and which, without any comment, I shall submit to your consideration, were as follows.

Experiment N° 30.

Having provided a quantity of water, which, by being exposed with a few green leaves in the sun, had acquired a greenish cast, and which I found was disposed to yield dephlogisticated

gified air in great abundance, I filled a globe, containing 46 cubic inches, with this water, and putting to it two healthy peach leaves, exposed the globe in the sun upon the 7th of September, from 11 o'clock in the morning till 2 o'clock in the afternoon (3 hours), when $\frac{7}{8}$ of a cubic inch of air was produced, which, proved with nitrous air, gave $1a + 3n = 1,52$, or 248 degrees.

A like globe, with fresh spring water and two peach leaves, exposed at the same time, furnished only $\frac{1}{8}$ of a cubic inch of air, which, on account of the smallness of its quantity, I did not submit to the test of nitrous air.

Experiment N° 31.

September 8. Very fine clear weather, but rather cold for the season. Three equal globes, A, B, and C, containing each 46 cubic inches, were filled as follows, and exposed in the sun from 9 o'clock in the morning till half an hour past 4 in the afternoon, when they were found to have produced air as under mentioned.

The globe A, filled with water, which, by being previously exposed in the sun for several days, with potatoes cut in thin slices, had turned green, furnished $\frac{2}{8}$ of a cubic inch of air of 299 degrees, or $1a + 3n = 1,01$. N. B. This water, before it was put into the globe, was strained through two thickneses of very fine Irish linen.

The globe B, filled with the same green potatoe water (strained as before) to which were added four middling-sized peach leaves, furnished $2\frac{1}{2}$ cubic inches of air of 320 degrees, or $1a + 4n = 1,80$.

The globe C, filled with fresh spring water, with four peach leaves, furnished $\frac{1}{8}$ of a cubic inch of air of 151 degrees, or which, proved with nitrous air, gave $1a + 2n = 1,49$.

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To ascertain the quantities and qualities of the airs remaining in the different waters used in this experiment, putting the globes separately over a chafing-dish of live coals, and making the water boil, taking care to hold the globe in such an inclined position as that the air separated from the water might be collected in the upper part of the globe, the airs produced were as follows.

	Quantity.	Quality.
By the green water in the globe A,	$\frac{5}{100}$ of a cubic inch	280 degrees
By the green water in the globe B,	$\frac{3}{100}$ - -	241
By the spring water in the globe C,	$\frac{1}{100}$ - -	68

The waters in these experiments were made to boil but for a moment; otherwise, it is probable, more air might have been separated from them.

Finding that fresh leaves, exposed to the action of the sun's rays, in water which had already turned green, caused pure air to be separated from the water in so great abundance, I repeated the experiment, only, instead of leaves, I now made use of a small quantity of *conferva rivularis*; when I had nearly the same result as with the leaves.

To ascertain the relative quantities and qualities of the airs yielded by the green water, when exposed with fresh leaves, and when exposed with raw silk; and also to ascertain, at the same time, how long leaves, exposed in green water, retain their power of separating air from it, I made,

Experiment N° 32.

Two equal globes, A and B (containing 46 cubic inches), the former (A) filled with green potatoe water, strained through linen, and four peach leaves; the latter (B) filled with the same potatoe water, strained in like manner, and 17 grains of

raw silk, were exposed from Sunday noon, September 10th, till Monday evening, the weather being cold, with many flying clouds, in all about 6 or 7 hours sun.

The airs produced were as follows.

	Quantity.	Quality.
By the globe A, with green water and 4 peach leaves	$2\frac{7}{8}$ cubic inches	292 deg.
By the globe B, with green water and 17 grs. of raw silk	$2\frac{1}{8}$ - -	307

Another globe (C), filled with green water *alone*, was exposed at the same time; but it was broken by an accident before the experiment was completed.

The two globes (A and B) with their contents, being again exposed from Tuesday noon till Thursday evening, yielded air as follows.

	Quantity,	Quality.
The globe A, with the peach leaves	$4\frac{47}{8}$ cubic inches	344 degrees
The globe B, with raw silk -	$4\frac{1}{8}$ - -	350

N. B. The weather on Tuesday and Wednesday was cold, with very little sunshine; but Thursday was a very fine warm day, when the greatest part of the air was produced. This air was removed and proved on Friday morning the 15th September.

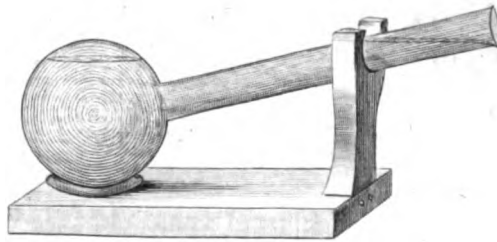
Perhaps all the appearances above described might be satisfactorily accounted for, by supposing the air produced in the different experiments to have been generated in the mass of water by the *green matter*; and that the leaves, the silk, &c. did no more than *assist it in making its escape*, by affording it a convenient surface to which it could attach itself, in order to its collecting itself together, and taking upon itself its elastic form.

The phænomena might likewise be accounted for by supposing the *green matter* to be a vegetable substance, agreeable to the hypothesis of Dr. PRIESTLEY, and that attaching itself to the surfaces of the bodies exposed in the water, as a plant is attached to its soil, it grows; and, in consequence of the exertion of its vegetative powers, the air yielded in the experiment is produced.

I should most readily have adopted this opinion, had not a most careful and attentive examination of the green water, under a most excellent microscope, at the time when it appeared to be most disposed to yield pure air in abundance, convinced me, that, *at that period*, it contains nothing that can possibly be supposed to be of a vegetable nature. The colouring matter of the water is evidently of an animal nature, being nothing more than the assemblage of an infinite number of very small, active, oval-formed animalcules, without any thing resembling *tremella*, or that kind of *green matter*, or water moss, which forms upon the bottom and sides of the vessel when this water is suffered to remain in it for a considerable time, and into which Dr. INGEN-HOUZS supposes the animalcules above-mentioned to be actually transformed.

But having finished the account of my experiments, I shall finish my letter, not daring to venture conjectures upon a subject so intricate in itself, and which is yet so new, and upon which the ablest philosophers of the age seem to be so much divided in opinion.





Boyle Sc.

XVI. *An Account of the Discovery of Two Satellites revolving round the Georgian Planet.* By William Herschel, LLD.
F. R. S.

Read Feb. 15, 1787.

THE great distance of the Georgian planet, and its present situation in a part of the zodiac which is scattered over with a multitude of small stars, has rendered it uncommonly difficult to determine whether, like Jupiter and Saturn, it be attended by satellites. In pursuit of this inquiry, having frequently directed large telescopes to this remote planet, and finding myself continually disappointed, I ascribed my failure to the want of sufficient light in the instruments I used; and, for a while, gave over the attempt.

In the beginning of last month, however, I was often surprised when I reviewed nebulæ that had been seen in former *sweeps*, to find how much brighter they appeared, and with how much greater facility I saw them. The cause of it could be no other than the quantity of light that was gained by laying aside the small speculum, and introducing the *Front-view*; an account of which has been inserted, by way of note, to the Catalogue of Nebulæ contained in the Philosophical Transactions, vol. LXXVI. p. 499.

It would not have been pardonable to neglect such an advantage, when there was a particular object in view, where an accession of light was of the utmost consequence; and I wondered

dered why it had not struck me sooner. The 11th of January, therefore, in the course of my general review of the heavens, I selected a *sweep* which led to the Georgian planet; and, while it passed the meridian, I perceived near its disk, and within a few of its diameters, some very faint stars whose places I noted down with great care.

The next day, when the planet returned to the meridian, I looked with a most scrutinizing eye for my small stars, and perceived that two of them were missing. Had I been less acquainted with optical deceptions, I should immediately have announced the existence of one or more satellites to our new planet; but it was necessary that I should have no doubts. The least haziness, otherwise imperceptible, may often obscure small stars; and I judged, therefore, that nothing less than a series of observations ought to satisfy me, in a case of this importance. To this end I noticed all the small stars that were near the planet the 14th, 17th, 18th, and 24th of January, and the 4th and 5th of February; and though, at the end of this time, I had no longer any doubt of the existence of at least one satellite, I thought it right to defer this communication till I could have an opportunity of seeing it actually in motion. Accordingly I began to pursue this satellite on Feb. the 7th, about six o'clock in the evening, and kept it in view till three in the morning on Feb. the 8th; at which time, on account of the situation of my house, which intercepts a view of part of the ecliptic, I was obliged to give over the chase: and during those nine hours I saw this satellite faithfully attend its primary planet, and at the same time keep on, in its own course, by describing a considerable arch of its proper orbit.

While I was chiefly attending to the motion of this satellite, I did not forget to follow another small star, which I was pretty

pretty well assured was also a satellite, especially as I had, on the night of the 14th of January, observed two small stars which were wanting the 17th, and again missed other two the 24th which had been noticed the 18th; but, whether owing to my great attention to the former satellite, or to the closeness of this latter, which was nearly hidden in the rays of the planet, I could not be well assured of its motion. Indeed, towards morning, when a change of place, in so considerable an interval as nine hours, would have been most conspicuous, the moon interfered with the faint light of this satellite, so that I could no longer perceive it.

The first moment that offered for continuing these observations was on February the 9th, when I saw my first discovered satellite nearly in the place where I expected to find it. I perceived also, that the next supposed satellite was not in the situation where I had left it on the 7th, and could now distinguish very plainly that it had advanced in its orbit, since that day, in the same direction with the other satellite, but at a quicker rate. Hence it is evident, that it moves in a more contracted orbit; and I shall therefore call it in future the first satellite, though last discovered, or rather last ascertained; since I do not doubt but that I saw them both, for the first time, on the same day, which was January the 11th, 1787.

I now directed all my attention to the first satellite, and had an opportunity to see it for about three hours and a quarter; during which time, as far as one might judge, it preserved its course. The interval which the cloudy weather had afforded was, however, rather too short for seeing its motion sufficiently, so that I deferred a final judgment till the 10th; and, in order to put my theory of these two satellites to a trial, I made a sketch on paper, to point out before-hand their situation

situation with respect to the planet, and its parallel of declination.

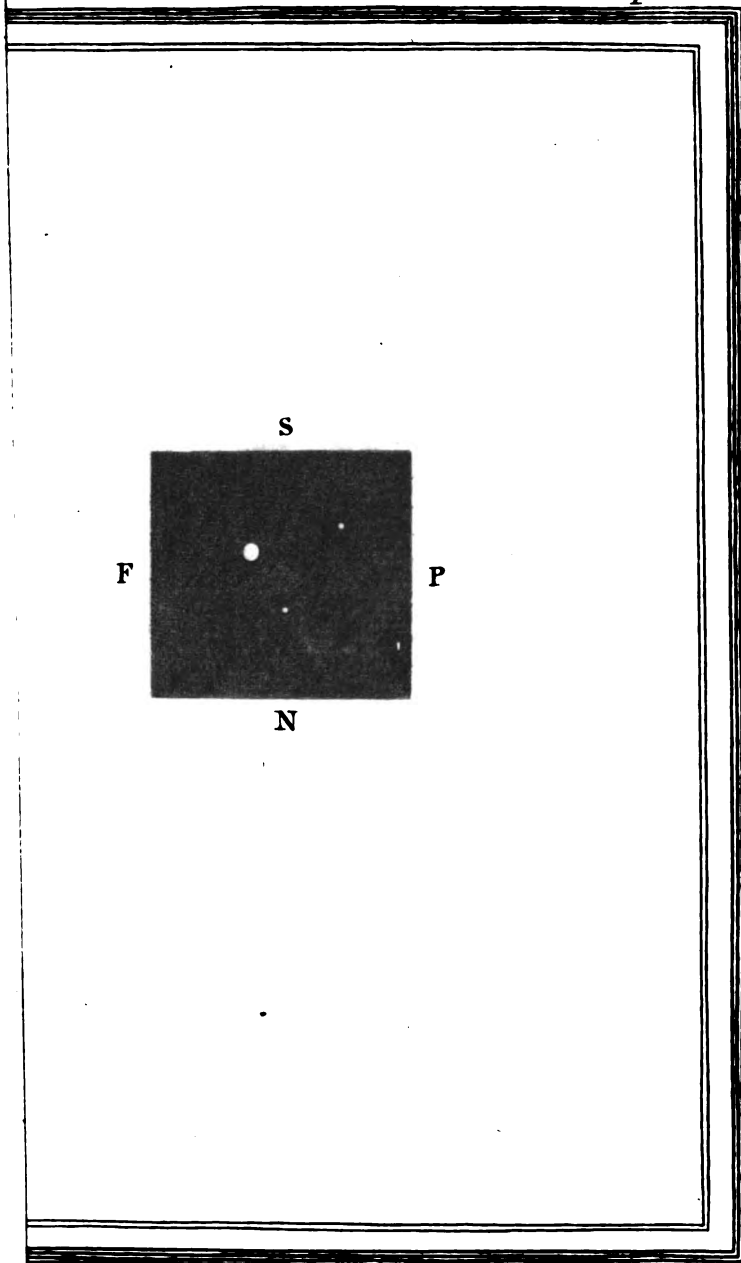
The long expected evening came on, and, notwithstanding the most unfavourable appearance of dark weather, it cleared up at last. And the heavens now displayed the original of my drawing, by shewing, in the situation I had delineated them, *The Georgian Planet attended by two Satellites.*

I confess that this scene appeared to me with additional beauty, as the little secondary planets seemed to give a dignity to the primary one, which raises it into a more conspicuous situation among the great bodies of our solar system.

For upwards of five hours I saw them go on together, each pursuing its own track; and I left them situated, about two o'clock in the morning on February the 11th, as they are represented in the figure, Tab. VII. The letters S, N, P, F, denote the south, north, preceding, and following parts of the heavens, as they are seen, by the *front-view*, in my telescope. The south preceding satellite is the second, or that whose motion was first ascertained; the other is that which moves in a smaller orbit, or what I have called the first satellite; and the direction of their motion is according to the order P, S, F, N, of the letters.

I have not seen them long enough to assign their periodical times with great accuracy; but suppose that the first performs a synodical revolution in about eight days and three-quarters, and the second in nearly thirteen days and an half.

Their orbits make a considerable angle with the ecliptic; but to assign the real quantity of this inclination, with many other particulars, will require a great deal of attention, and much contrivance: for, as estimations by the eye cannot but be extremely fallacious, I do not expect to give a good account of
their



their orbits till I can bring some of my micrometers to bear upon them ; which, these last nights, I have in vain attempted, their light being so feeble as not to suffer the least illumination, and that of the planet not being strong enough to render the small silk-worm's threads of my delicate micrometers visible. I have, nevertheless, several resources in view, and do not despair of succeeding pretty well in the end.

W. HERSCHEL.

Slough, near Windsor,
February 11, 1787.



XVII. Remarks on Mr. Brydone's Account of a remarkable Thunder-storm in Scotland. By the Right Honourable Charles Earl Stanhope, F. R. S.

Read February 15, 1787.

§ 1. **N**O storm of lightning has ever produced effects more curious to contemplate than those related by Mr. BRYDONE, in his Letter to the President of this Society. That account contains facts of such consequence, and so perfectly inexplicable by the principles of electricity which are commonly received, that it undoubtedly deserves particular attention.

It appears, that a man (named "JAMES LAUDER") sitting on the fore-part of a cart drawn by two horses, was suddenly struck dead, as also the horses that he was driving, and that the cart itself was much injured by electrical fire, although no lightning fell at, or near, the place where this accident happened.

§ 2. Before I attempt to account, by the laws of electricity, for this remarkable phenomenon, it may not be improper just to take notice, that few facts of this kind have ever been better authenticated than this is.

It appears, first, that a man, who was sitting upon the fore-part of another cart, only twenty-four yards behind the cart that was struck, "had LAUDER, his cart and horses, full in view when they fell; he was stunned by a loud report, and saw his companion, his horses and cart, fall to the ground; " he

“ he immediately ran to his assistance, but found him quite “ dead ; he perceived ” (at the time of the accident) “ no flash “ or appearance of fire.”

It also appears, that another man, a shepherd of St. Cuthbert's farm, was also a witness of this event. He was distant from LAUDER “ between two and three hundred yards, and was “ looking at the two carts, when he was stunned by a loud “ report, and at the same instant saw the first of the carts fall “ to the ground. He saw no lightning, nor appearance of fire “ whatever.”

The concurrent testimony of these two men is confirmed by PATRICK BRYDONE, Esq. who lives “ at a small distance ” from the spot where LAUDER was killed : and Mr. BRYDONE relates, that a storm appeared far off ; and that he, and some company in his house, were “ suddenly alarmed by a loud “ report, for which they were not prepared by any preceding “ flash.” There is the greater weight to be given to this account of Mr. BRYDONE, as it so happened, that he was just then “ observing the progress of the storm, at an open win- “ dow, in the second story of his house,” and making the company “ observe, by a stop-watch, the time that the sound “ took to reach them.”

It is extremely fortunate, that a gentleman, fond of philosophical enquiries, should have been upon the spot at the time of this accident, to give to the world so clear and interesting an account.

§ 3. That the death of LAUDER and of the horses was *not* occasioned by any *direct main stroke of explosion* from a thunder-cloud, either positively or negatively electrified, is evident ; since no lightning whatever did pass from the clouds to the

earth (or from the earth to the clouds) at the place where they were killed.

§ 4. It is equally evident (and for the very same reason) that they were not deprived of life by any *transmitted main stroke of explosion*, either positive or negative.

§ 5. It is also obvious, that the *lateral explosion* was not the cause of this mischief; for the *lateral * explosion* does always proceed immediately from the main stroke itself; and therefore there can exist no *lateral explosion*, in the case when there is no main stroke whatever.

§ 6. It might, perhaps, be supposed, that LAUDER and the two horses were suddenly suffocated by the sulphureous smell that often accompanies electricity.

But though the death of the man and of the horses might (by some) be attributed to this cause, the sulphureous smell will no wise account for “many pieces of the coal having been “thrown out, to a considerable distance, all round the cart;” and for the “splinters” (from the wood-work of the cart) that were “thrown off in many places,” as appears, by Mr. BRYDONE's account, to have been the case.

§ 7. Some persons may imagine that LAUDER and the horses might have been killed, and that the other effects above-mentioned might be accounted for, by some sudden and very violent commotion in the air, which commotion the thunder might be supposed to produce, though unaccompanied with any flash at that place; in a manner, in some small degree, similar to the fatal wounds that sometimes have been known to have been given by the air having been suddenly displaced

* See Dr. PRIESTLEY's excellent Paper in the sixtieth volume of the Philosophical Transactions, page 192 et seq. See also my Principles of Electricity, § 386.

by a cannon ball, in its passage through that atmospherical fluid, though the cannon ball itself had evidently neither struck the person wounded, nor even grazed his cloaths.

The "dust that rose at the place" might be brought as an additional argument in favour of the opinion, that a sudden and violent commotion of the air did occasion the effects produced.

But such an explanation (vague as it is) would not account for the "marks of fusion" on the iron of the wheels; nor for "the hair of the horses having been singed;" nor for "the skin of LAUDER's body having been burnt" in many places.

§ 8. I think, from the different circumstances of this case, that it is clear to demonstration, that the effects produced proceeded from electricity; and that *no electrical fire did pass* immediately either from the clouds into the cart, &c. or from the cart, &c. into the clouds.

From "the circular holes" in the ground ("of about "twenty inches in diameter") the respective "centers of "which were exactly in the track of each wheel," and the corresponding "marks of fusion" on the iron of the wheels, which marks "answered exactly to the center of each of the "holes;" it is evident, that the electrical fire did pass (from the earth to the cart, or from the cart to the earth) through that part of the iron of the wheels which was in contact with the ground.

From the "splinters that had been thrown off, in many "places, particularly where the timber of the cart was connected by nails or cramps of iron," and from the various other effects mentioned in Mr. BRYDONE's Paper, it is moreover evident, that there was a violent motion of the electrical fluid in
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all, or (at least) in different parts of the catt, and of the bodies of the man and horses, although there were no lightning.

§ 9. Wonderful as these combined facts may appear, and uncommon as they certainly are in this country, they are, nevertheless, easy to be explained by means of that particular species of electrical shock, which I have distinguished in my Principles * of Electricity (published in 1779) by the appellation of the “*electrical returning stroke* :” and although at the time I wrote that Treatise, I had it not in my power to produce any instance of persons or animals having been killed in the very peculiar manner since related in Mr. BRYDONE's Paper; I did, however (from my experiments mentioned in that Book), venture to assert, with confidence, that †, “if
“ persons be strongly superinduced by the electrical atmosphere
“ of a cloud, they may (under circumstances similar to those
“ explained in that Treatise) receive a very strong shock, be
“ knocked down, or be even killed, at the instant that the
“ cloud discharges, with an explosion, its electricity, whether
“ the lightning falls near the very place where those persons
“ are, or at a very considerable distance from that place, or
“ whether the cloud be positively or negatively electrified.”

And I moreover stated that ‡, “whether the distance between the person so circumstanced, and the place where the lightning falls, be fifty or an hundred yards, or one mile, or two miles, or three miles, or more, the truth of the ge-

* See Principles of Electricity, containing divers new Theorems and Experiments, together with an Analysis of the superior Advantages of high and pointed Conductors; by CHARLES VISCOUNT MAHON, F. R. S. from § 202. to § 347. inclusively.

† See Principles of Electricity, § 311.

‡ See Principles of Electricity, § 313.

“ neral

“ neral proposition there laid down would not be any wise
“ affected.”

I have also explained, in that Treatise, how a still more singular effect might be produced, namely, how * “ an explosion, which happens in one place, may cause in a second place (at a very considerable distance from the first place) a sudden returning stroke, which may knock down, or even kill, persons and animals at that second place; at the same time that other persons, or other animals, situated in a third place, that is even immediately between the first place where the lightning falls, and the second place (just mentioned) where the shock of the returning stroke happens, shall receive no detriment whatever.”

§ 10. Having, in my Principles of Electricity †, explained at large the nature of the *returning stroke*, I will not trespass upon the time of this Society, by repeating the account of any of the various experiments that I made, to prove the possible existence of such an electrical stroke; but I will, at once, apply the general laws, which I have (therein) laid down, to the particular phenomena related by Mr. BRYDONE.

But, before I speak of the accident of LAUDER, which appears to me to have been occasioned by a *returning stroke*, proceeding from an *assemblage* of clouds, I will say a few words upon one or two other facts, mentioned in Mr. BRYDONE's account.

§ 11. Mr. BRYDONE informs us, that “ the shepherd belonging to the farm of Lennel-Hill was in a neighbouring field, when he observed a lamb (only a few yards from him) drop down, although the lightning and claps of thunder were,

* See Principles of Electricity, § 314.

† See Principles of Electricity, from § 202 to § 347; inclusively.

“ then, at a great distance from him. He ran up immediately,
 “ but found the lamb quite dead; nor did he perceive the least
 “ convulsive motion, or symptom of life remaining, although,
 “ the moment before, it appeared to be in perfect health.”

This effect is so precisely similar to those explained in my Principles of electricity, and particularly to that mentioned in section 328, that it is quite unnecessary to enlarge upon it. I will only observe, that such an electrical *returning stroke* as that by which this lamb was destroyed (namely, a returning stroke, which happens at a place where there is neither lightning nor thunder near) belongs to the most simple class of returning strokes; and that it may be produced by the sudden removal of the elastic electrical pressure of the electrical atmosphere of a *single* * main cloud, as well as by that of an *assemblage* † of clouds.

It appears by Mr. BRYDONE's account, that the shepherd, who saw the lamb fall, was near enough to it to feel, in a small degree, the electrical returning stroke “ at the same time” that the lamb dropped down.

§ 12. Mr. BRYDONE further relates, that “ a woman making hay near the banks of the river fell suddenly to the ground; and called out to her companions, that she had received a violent blow on the foot, and could not imagine from whence it came.”

This blow was, unquestionably, the *electrical returning stroke*.

When a person, walking or standing out of doors, is knocked down or killed by the *returning stroke*, the electrical fire must rush in, or rush out, as the case may be, through that

* See Principles of Electricity, § 312.

† See, in my Principles of Electricity, the end of the note to § 312.

person's

person's feet*, and through them *only*; which would not be the case, were the person to be knocked down or killed by any main stroke of explosion, either positive or negative.

These things are evident. But the manner in which LAUDER and the two horses were killed is not quite so evident, though it appears to me to be very easily accounted for.

I will now state, in what manner I conceive that the clouds may have been placed, when the effects mentioned in Mr. BRYDONE's Paper were produced.

§ 13. I must premise that, by that account, it appears, that the cloud was many miles in length; inasmuch, as just before the "loud report," the lightning was at a very "great distance:" for Mr. BRYDONE "observed, by a stop-watch, that "the time that the sound took to reach him was generally from "25 to 30 seconds."

Mr. BRYDONE relates, that the "loud report resembled the "firing of several muskets, so close together, that the ear "could hardly separate the sounds, and was followed by no "rumbling noise like the other claps."

This description indicates, that the electrical explosion was not far distant; and also shews, that it was not extremely near: for, if the explosion had been extremely near, the ear could not at all have separated the sounds.

§ 14. Now let us suppose a cloud, eight, ten, or twelve miles in length (be the same more or less) to be extended over the surface of the earth, in the situation represented by ABC in the figure. (See Tab. VIII.)

And let another cloud (as represented by DEF) be situated between the above-mentioned cloud and the earth.

* See Principles of Electricity, from § 322. to § 327. inclusively.

Let the two clouds be supposed to be charged (for instance) with the *same* kind of electricity, and to be both positive.

Let us further suppose, that the lower cloud DEF be near the earth, only a little beyond the striking distance; and let a man, cart and horses, be situated at L, under that part (E) of the cloud which is the nearest to the earth.

Now, let us suppose this cart to be ascending an hill, and to be in the situation described by Mr. BRYDONE, namely, to have "almost gained the highest part of the ascent," and to be followed by "another cart" (M) lower down the hill.

Let us suppose also, that the two clouds DEF and ABC be near each other, perpendicularly over the place where the cart L was situate (as at DA).

And let the remote end C, of the upper cloud ABC, approach the earth, within the striking distance, and suddenly discharge its electricity into the earth at G.

§ 15. Things being situated as above described, let us examine what consequences must follow.

First, when the upper cloud ABC discharges its electricity into the earth at G, the lower cloud DEF must immediately discharge its electricity into the upper cloud at the place DA, which is directly or nearly over the cart L.

This accounts for the "loud report" of thunder that was unaccompanied by lightning at L or at M. The *report* must be loud, from its being near; but no *lightning* could be perceived at L or M, by reason of the thick thunder cloud DEF being situated immediately between the spectator at M and DA, the place between the two clouds where the lightning was.

§ 16. Secondly, as the lower cloud DEF did gradually approach towards the earth at L, so as to come finally into the situation

situation represented in the figure; the earth at L must, of course, become superinduced by the elastic electrical pressure of the electrical atmosphere of the thunder-cloud; which superinduced elastic electrical pressure must gradually have increased as the cloud came closer to the earth, and approached nearer to the limit of the striking distance.

§ 17. 'Consequently, if any conducting body (not having any prominent conducting points) were to be placed at L, upon the surface of the earth, and to be there *electrically insulated*; then such conducting body, by the laws of electricity, must, at its upper extremity (namely, the part nearest to the *positive* cloud) become *negative*; at its lower extremity, it must become *positive*; and, at a certain intermediate point, it will be neither *plus* nor *minus*.

So that this insulated conducting body, thus situated, will be in three opposite states at one and the same time; that is to say, that it will be, at the same instant, *positively* electrified, *negatively* electrified, and not electrified at all.

This proposition I have rigorously demonstrated in *my* Treatise * on Electricity.

§ 18. But if this conducting body, on the surface of the earth, be *not insulated* †, or be but imperfectly insulated, then the *whole* of such body (from its being immersed in the electrical atmosphere of the *positive* cloud) will become *negative*; because part of the electricity of this conducting body will, in this case, pass into the earth. And this conducting body will become the more *negative*, as it becomes the more deeply immersed into the dense part of the elastic electrical atmosphere of the approaching thunder-cloud.

* See Principles of Electricity, from § 55 to § 74, inclusively.

† See Principles of Electricity, § 182 and § 183.

§ 19. Now, when the positive cloud DEF (in the manner above stated) comes suddenly to discharge, with an explosion, its superabundant electricity into the other cloud ABC, then the elastic electrical atmosphere of the cloud DEF will cease to exist; consequently, the electrical fluid, which had been gradually expelled into the common stock, from the conducting body situated (at L) upon the surface of the earth, must (by the sudden removal of the superinduced elastic electrical pressure of the electrical atmosphere of the thunder-cloud) suddenly return from the earth into the said conducting body, producing a violent * commotion, similar to the pungent shock of a *Leyden jar* in its sensation and effects.

This is what I call the electrical returning + stroke.

§ 20. It was by such a returning stroke that LAUDER and the horses that he was driving were killed, they having become strongly *negative* † previous to the explosion.

LAUDER was "fitting" when he was struck dead; and his legs appear to have been "hanging over the fore-part of the cart, at the time of the explosion." The returning stroke, therefore, could not issue into his body through his legs; and this accounts for the "skin of his legs" not having been at all "burnt or shrivelled," as the skin was on many other parts of his body. And it likewise shews the reason, why the "zig-zag line" on LAUDER's skin (which was terminated at one end by the chin) "did not extend lower than the thigh."

* See Principles of Electricity, § 304 and § 603.

† See Principles of Electricity, from § 202 to § 208, inclusively, and § 387.

‡ Note, I have here supposed the thunder-cloud (and of course its electrical atmosphere) to have been *positive*; but similar effects would have been produced if the thunder-cloud had been *negative*; only, in that case, LAUDER and the horses would have been strongly positive, previous to the explosion.

§ 21. Mr. BRYDONE mentions, that “ the hair of the horses “ was much singed over the greatest part of their bodies; but “ was most perceptible on the belly and legs.” This effect is easily accounted for by the returning stroke; for the lower part * of the bodies of those animals must of course have been more strongly affected than any other part, as the electrical fire from the earth must suddenly have rushed into their bodies through “ their legs, which had made a deep impression “ in the dust.”

§ 22. The various effects produced on the cart may be explained, with equal facility, by means of the returning stroke; and I have stated, in my Principles † of Electricity, “ not “ only in what manner persons and animals may be destroyed, “ but how” inanimate bodies, such as “ particular parts of buildings, may be considerably damaged by an electrical returning “ stroke, namely, all those parts where there is, in any kind “ of conducting substance or substances (upon which a strong “ elastic electrical pressure is superinduced), any kind of electrical interruption, across which the returning electrical fire “ might suddenly strike, and might thereby rend and destroy “ all the bodies that it might meet with in its passage.”

Mr. BRYDONE relates, that “ splinters had been thrown off “ in many places, particularly where the timber of the cart was “ connected by nails or cramps of iron.” The electrical interruption between these pieces of metal, across which interruption the returning electrical fire did suddenly strike, accounts for its rending the bodies that it met with in its passage.

* See, in my Principles of Electricity, the note to § 594, where somewhat of a similar effect is mentioned.

† See Principles of Electricity, § 333, § 334, and § 347.

It is also evident, that it was the electrical returning fire, that produced the "marks of fusion" on that part of the iron of the wheels which was in contact with the ground; inasmuch as the whole electricity, that, at the instant of the explosion, returned into the cart, did enter at those places.

§ 23. No person, the least versed in the principles of electricity, can hesitate to assent to the proposition, that the *electrical returning stroke* must exist, under circumstances similar to those explained above. But it may be objected to me, that although all the aforesaid effects of a returning stroke might take place in a small degree, yet those effects could not have been sufficiently powerful to have killed LAUDER, the horses, and the lamb, or to have melted the iron of the cart-wheels; especially, considering the small quantity of electrical fluid that is contained in the body of a man, of a lamb, or of a horse; or that is contained in any body of the size of a common cart; that is to say, considering the small quantity of electrical fluid that could, by being disturbed, have produced the *returning stroke*.

To this objection (plausible as at first sight it may appear) I conceive, I have given a complete answer in my *Principles of Electricity*, from section 337 to section 347, inclusively; and also from section 592 to section 606, inclusively; but it may not be improper to add a few words to what I have already said upon that part of the subject.

§ 24. No legitimate conclusion can be drawn from premises that are not proved: therefore, no person can legitimately conclude, that the force of a returning stroke must always be weak, when produced by the disturbed electrical fluid of a man's body, by reason that a man's body contains but a small quantity of electricity: for, it has never been proved, that a
man's

man's body does contain only a *small* quantity of electrical fluid; neither is there the smallest reason to believe such an hypothesis, which appears, on many accounts, to be completely erroneous. And, if that hypothesis be erroneous, the objection to the strength of an electrical returning stroke remains perfectly unsupported by argument.

When a body is said to be *plus* or *positive*, it simply means, that the body contains *more* electricity than it does in its un-electrified, that is to say, natural state; but does not signify, that such body is completely saturated * with electricity. In like manner, when a body is said to be *minus* or *negative*, it only signifies, that the body contains *less* than its natural share of electricity; but does not imply, that such body is *completely exhausted* of the electricity which it contains in its natural state.

Now, the strength of natural electricity is so immense, when compared to the very weak effects of our largest and best contrived electrical machines, that I conceive, that we cannot, by means of artificial electricity, expel, from a man's body, the thousandth (or perhaps even the ten thousandth) part of the electrical fluid which it contains, when in its natural state.

§ 25. That hypothesis, by which natural phænomena are easily accounted for, has a better claim to our attention than an opposite hypothesis, which prevents those phænomena from being intelligibly explained.

There is no reason whatever for concluding, that any electrical machine of any *given* size is capable of rendering a conducting body either *completely plus*, or *completely minus*; but far otherwise. And it would have been as logical, for any person, some years ago (when electrical machines were not brought to

* See Principles of Electricity, § 342.

their

their present state), to have maintained, that those very imperfect machines were capable of rendering a conducting body *completely positive*, or *completely negative*, as for us, in the year 1787, to conclude, that we (by our still imperfect machines) have attained the limit of electrical exhaustion, or condensation.

We evidently have not, with our machines, even approached the limit of electrical strength, particularly in respect to the *returning stroke*: for it is remarkable, that (by the laws of electricity *) the strength of the electrical *returning stroke*, near the limit of the striking distance, does increase in a "greater ratio," than the strength of the main stroke from the charged body, producing the elastic electrical atmosphere superinduced.

§ 26. For example, let the returning stroke be attempted to be produced, by means of a metallic prime conductor of 20 or 21 inches in length, and of about two inches in diameter; and by means of another metallic body of equal dimensions, placed parallel to the prime conductor, just out of the limit of the striking distance; and let the prime conductor be charged, by means of one of the common glass globes, of less than nine inches in diameter.

The returning stroke, in this case, will not only be considerably weaker than a spark taken from the prime conductor, but it will be so extremely weak, that it can hardly be said even to exist.

§ 27. Whereas, if the experiment be made in a manner exactly similar, by means of a *large* glass cylinder (instead of the small globe) and by means of a metallic prime conductor, of about three feet four inches long †, by nearly four inches and a

* See my Principles of Electricity, § 340 and § 341.

† It is better if the prime conductor, and the other metallic body, be still larger.

half diameter*; and also by means of another metallic body of equal dimensions with this prime conductor; then, there will be no kind of comparison between the strength of the *returning stroke* obtained out of the striking distance of the prime conductor, and the strength of the *main stroke* received immediately from the prime conductor, the sharpness and pungency of the *returning stroke* being so much superior. The returning stroke in this case is like the sudden discharge of a weakly electrified *Leyden jar*, provided that due attention be paid to the four rules for obtaining a very strong returning stroke, as laid down in section 307 of my Principles of Electricity.

§ 28. When I performed the experiment of the returning stroke, by means of a still stronger electrical apparatus, the commotion † felt was similar to that of a *Leyden jar*, strongly electrified, suddenly discharged through my body; so that, having taken the returning stroke eight or ten times one morning (without having taken the *main stroke* a single time that day), I felt a considerable degree of pain across my chest during the whole evening, and a disagreeable sensation in my arms and wrists all the next day.

I have also found, that (by an advantageous disposition of the apparatus) metal ‡ may be melted by means of the *electrical returning stroke*, not only entirely out of the *striking distance*, but even without any communication with the common stock, although the conducting body, from which issued the electrical

* See Principles of Electricity, § 593, 594, and 595.

† See Principles of Electricity, § 304 and § 310.

‡ See Principles of Electricity, from § 603 to § 606, inclusively.

fluid that formed the *returning stroke*, had less than *twenty-seven* square feet of surface in contact with the air.

§ 29. The fact is, that in the case of the *returning stroke*, it is not so much upon the *quantity* of electrical fluid, as upon the *velocity* * of that fluid, that the strength of that stroke depends; therefore, the strength of the *returning stroke* depends less upon the *quantity of surface* used, than upon the *strength* of the electrical pressure of the elastic electrical atmosphere, superinduced upon the body struck, previous to the explosion.

But, the electrical pressure of the elastic electrical atmosphere of the great thunder-cloud in Scotland must have been immense, when compared to the electrical pressure of the elastic electrical atmosphere of a metallic prime conductor, of whatsoever shape; consequently, it is not at all surprising that LAUDER should have been killed (or that the other effects, related by Mr. BRYDONE, should have been produced) by the *returning stroke*; inasmuch as it is not surprising, that *effects* should be proportionate to the *causes* by which they are respectively produced.

§ 30. Mr. BRYDONE relates, that "LAUDER's cart was "higher on the bank" than the cart that followed him; which, in some degree, accounts for the man, sitting on the other cart, *not* having felt the *returning stroke*. But, that is to be accounted for in another way, namely, by supposing the cloud to have been pending nearer to the earth, over the spot where

* See Principles of Electricity, from § 592 to § 602, inclusively; and particularly § 601.

LAUDER

LAUDER* was killed, than over the place where his companion was: for, I have shewn, in my Treatise upon Electricity †, that, in order for a person to receive a dangerous returning stroke, such person should be immersed, not merely in the electrical atmosphere of the 'thunder-cloud', but in the dense part of the cloud's electrical atmosphere.

The fact above alluded to may also be accounted for in the following way, *viz.* by supposing that the second cart were either better connected with the common stock, or that it were better insulated, than LAUDER's cart: for, I have shewn, in my Principles of Electricity ‡, (what is very remarkable, namely,) that *either* of these two *opposite* circumstances will weaken the force of a returning stroke prodigiously. Now, Mr. BRYDONS mentions, that there had been an "almost total want of rain" for many months. He also says, that "the ground" (at the place where LAUDER was killed) "was remarkably dry; and of a gravelly soil." This state of the ground was particularly adapted to the production of the electrical returning stroke, when produced upon the large scale of nature, where the elastic electrical pressure is so powerful.

§ 31. The account which Mr. BRYDONS has given of this thunder-storm in Scotland is not more curious than it is instructive.

* See Principles of Electricity, § 318.

† See, in my Principles of Electricity, Experiments 38, 39, and 40, from § 280 to § 296, inclusively. See also § 312, § 318, § 334, and § 307.

‡ See Principles of Electricity, from § 248 to § 330, inclusively; and see particularly § 307.

In part XIX. of my *Principles of Electricity* *, I have enumerated “ eleven necessary requisites ” in erecting conductors to secure buildings against damage by lightning. The ninth requisite is †, “ that there be neither large nor prominent bodies “ of metal, upon the top of the building proposed to be “ secured, but such as are connected with the conductor ” (and consequently with the common stock) “ by some proper metallic communication.” And in section 538, I state, that the “ consideration of the electrical returning stroke fully evinces the “ utility of such precaution.” The circumstances mentioned by Mr. BRYDONE, that “ splinters had been thrown off in “ many places, particularly where the timber of the cart was “ connected by nails, or cramps of iron,” still more fully proves that such precaution is right.

§ 32. The tenth ‡ necessary requisite in erecting conductors, mentioned in my *Principles of Electricity*, is, “ that there be “ a sufficient number of rods.” And in section 542 I state that, “ the highest parts of a building, the most elevated “ ridges, all the very prominent stacks of chimneys, and all “ the most salient angles, ought, in order for the building to “ acquire the greatest degree of security, to be armed with “ an high, tapering, and acutely pointed metallic conductor, “ properly connected with the common stock. And upon edifices of great importance (especially magazines of gunpowder) the pointed conductors ought never to be above

* See *Principles of Electricity*, from § 519 to § 544, inclusively; and particularly see § 519.

† See *Principles of Electricity*, § 519 and § 538.

‡ See *Principles of Electricity*, § 519; and from § 539 to § 542, inclusively.

“ forty or fifty feet asunder; and, if they were to be at still smaller distances asunder, the security they would afford would be still more perfect.”

The reason of this precaution is fully explained in many parts of the above-mentioned Treatise, particularly in section 423*; and the circumstance, very worthy of observation, related by Mr. BRYDONE, namely, that the distance was only “ about *twenty-four yards*,” between LAUDER, who was killed, and his companion, who “ was sensible of no shock, nor uncommon sensation,” does clearly demonstrate the propriety of this precaution, of erecting *several* † conducting rods upon an extensive building.

This information must be particularly interesting to the Board of Ordnance, on account of the security of their magazines, particularly their powder magazines at Purfleet. The

* See also Part XVIII. of my Principles of Electricity (from § 494 to § 518 inclusively), where I have clearly demonstrated this proposition, namely, that, “ *high and pointed metallic conductors* [when properly constructed, and when made to “ communicate completely with the common stock] tend not only to prevent “ a *main stroke of lightning* and the *lateral explosion*; but tend likewise most powerfully to prevent any dangerous *electrical returning stroke* whatever from taking “ place near that part of the edifice upon which they are erected. So admirable, “ and so extensive, is the principle upon which is founded this simple and most “ incomparable invention!”

† See also (in the volume LXVIII. of the Philosophical Transactions, part I. p. 313. et seq.) the “ Report of the Committee, appointed by the Royal Society,

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“ to consider of the most effectual Method of securing the Powder Magazines at Purfleet against the Effects of Lightning, in compliance with the Request of the “ Board of Ordnance;” in which Report, *several* high and acutely-pointed conductors, properly connected with the bottom of the wells, are proposed to be erected; and other precautions, founded on a like principle, are recommended.

security of those magazines is a great national object, not only on account of their importance in time of war ; but, also on account of their vicinity to the City of London. For, from the immense quantity of gunpowder they contain (when full), their situation upon the river Thames, and their being within the distance of only a few miles of London, those magazines being blown up would probably produce, in the Metropolis, a violent shock, like that of an earthquake.



XVIII. *Concerning the Latitude and Longitude of the Royal Observatory at Greenwich; with Remarks on a Memorial of the late M. Cassini de Thury. By the Rev. Nevil Maskelyne, D. D. F. R. S. and Astronomer Royal.*

Read Feb. 22, 1787,

M E M O I R E

Sur la jonction de Douvres à Londres.

Par M. CASSINI DE THURY, Directeur de l'Observatoire Royal; de la Société Royale de Londres, &c.

IL est intéressant pour le progrès de l'astronomie que l'on connaisse exactement la différence de longitude et de latitude entre les deux plus fameux Observatoires de l'Europe; et quoique les observations astronomiques faites depuis un siècle offrent un moyen assez exact pour parvenir à cette recherche, il paraît cependant que l'on n'est point d'accord sur la longitude de Greenwich à onze seconds près, et sur sa latitude à quinze secondes.

L'on a reconnu par les opérations trigonométriques exécutées en France, au Nord, et au Pérou, que sur l'étendue d'un degré du méridien ou de 57 mille toises, l'on se trompait à peine de dix toises, ce qui a été prouvé par des bases mesurées à l'extrémité des suites de triangles; ainsi sur la distance de Douvres à Londres, qui est de 49800 toises ou environ, on ne pourrait se tromper de 120 toises, qui répondent à onze secondes en longitude.

M. CASSINI a déjà publié, dans le livre de la Méridienne Véri-
fiée, les opérations par lesquelles l'on a déterminé la distance de
Calais à la grosse tour de Douvres de 18241 toises par un premier
triangle, et de 18243 toises par un second triangle; on aurait cette
distance avec une plus grande exactitude en observant les angles
conclus à Douvres, qui sont fort aigus. M. CASSINI a décou-
vert des côtes de France plusieurs objets sur les côtes d'Angle-
terre, qui seront visibles de la tour de Douvres; et sur cette
première base on établirait une suite de quelques triangles
jusqu'à Londres, dont le nombre et la grandeur dépendent de
l'exposition des objets compris dans la direction de Douvres à
Londres.

M. CASSINI ne doute point que ce projet ne soit agréé d'un
Souverain qui aime les sciences, qui non content des découvertes
du célèbre Cook vient d'ordonner un second voyage autour du
monde, et que la Société Royale ne charge un de ses Membres
de l'exécution; et dans le cas où ses occupations l'empêcheraient
de s'y livrer, qu'elle ne permît à M. CASSINI de s'en charger.
L'honneur qu'elle lui a fait de l'associer à un corps aussi re-
spectable ferait un titre pour lui accorder sa confiance. M.
CASSINI a profité du voyage du Roi en Flanders en 1748 pour
joindre les triangles de la méridienne à ceux de SNELLIUS en
Hollande; en 1762 il a prolongé la perpendiculaire de Paris
jusqu'à Vienne en Autriche. La branche qui s'étendra jusqu'à
Londres fera la troisième, et formera la jonction des deux plus
belles villes de l'Europe.

THE preceding Memorial of the late M. CASSINI DE THURY was put into my hands by Sir JOSEPH BANKS, our President, on the 28th of April, 1785, desiring me at the same time to give an answer to it. Happy if I can solve the doubts entertained by the late Royal Astronomer of France concerning the latitude and longitude of this Royal Observatory, and at the same time do justice to the memories of my learned predecessors, and to myself, I shall give an account of the principal operations that have been performed here for ascertaining those points, and then add my own remarks to elucidate the subject and reconcile the difficulties in question.

Had Dr. BRADLEY lived longer, for the benefit of astronomy, to publish his valuable observations, or had they been since published by another hand, which unfortunately they hitherto have not, these remarks might have been unnecessary, and perhaps even the occasion for them might never have occurred; as it would have then appeared upon what foundation the latitude of this Observatory had been established, and what differences of meridians between Greenwich and the other principal Observatories of Europe resulted from the observed eclipses of Jupiter's satellites and other celestial phenomena.

However, having formerly been apprised by Dr. BRADLEY himself of several particulars of moment relative to his observations, and particularly of the method which he used for settling his latitude and refractions, after he became possessed of the new instruments in 1750, and being assisted with some of his manuscript calculations, with the addition of my own observations, I flatter myself I can throw the light wanted on the
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question, and obviate the principal difficulty, that relative to the difference of latitude of Greenwich and Paris, and reduce the difference of meridians within smaller limits, notwithstanding Dr. BRADLEY's original observations had been removed from this Observatory, in which they were made, before I came here, and have not yet been restored to it.

Dr. BRADLEY having been furnished by Government in the year 1750 with a brass mural quadrant of eight feet radius, constructed by that excellent artist Mr. JOHN BIRD, an instrument far superior to any before used in the practice of astronomy, assiduously observed the pole star and other stars lying to the north of the zenith with it for upwards of three years, and then removed it to the opposite side of the wall, making it change place with the iron quadrant of the same radius constructed by Mr. GRAHAM, likewise an excellent instrument, though inferior to this, and commenced a regular series of observations of the sun, planets, and fixed stars, which have been ever since continued in the same manner. Moreover, the temperature of the air, shewn by the barometer and thermometer, is affixed to each observation; and the zenith point of the quadrant settled from time to time by the help of a zenith sector of $12\frac{1}{2}$ feet radius, turned alternately contrary ways, the same with which Dr. BRADLEY had before made his two useful and admirable discoveries of the aberration of light and the nutation of the earth's axis.

By the observations of the pole star and other circumpolar stars, above and below the pole, Dr. BRADLEY got the apparent zenith distance of the pole; by the apparent and equal zenith distances of the sun at the two equinoxes, having at the same time opposite right ascensions, as found from comparing his observed transits over the meridian with those of

fixed stars, after the manner used by Mr. FLAMSTEED for deducing the right ascensions of the fixed stars, he found the apparent zenith distance of the equator, which lessened by parallax and added to the apparent zenith distance of the pole gave a sum less than 90° by the sum of the two refractions belonging to the pole and meridian zenith distance of the equator. But he remarked, that the difference of refractions, belonging to these zenith distances, would come out the same within 2 or 3'' by any of the best tables then extant, whether deduced solely from observations, or partly from observations and partly from theory. The sum and difference of refractions answering to the pole and equator being thus given, the refractions themselves are given, the greater of which added to the apparent zenith distance of the equator gives the latitude of the place, and the less refraction added to the apparent zenith distance of the pole gives the co-latitude.

He afterwards, from the consideration that the refractions at the pole and equator may be taken without sensible error as the tangents of the zenith distances, according to Mr. THOMAS SIMPSON's theory of refractions in his Mathematical Dissertations, divided more accurately the sum of the refractions at the pole and the equator into the just parts answering to each zenith distance, and thereby found the latitude with more exactness. In this manner he found the latitude of the Royal Observatory to be $51^\circ 28' 39''\frac{1}{4}$, and the mean refraction at $45^\circ 3'$ to be $57''$, the barometer standing at 29,6 inches, and the thermometer of FAHRENHEIT's scale at 50° .

But, not to let a matter of so much consequence rest on my assertion or memory, when further proof can be given of it, I have by me, in the hand-writing of Dr. BRADLEY, among other particulars, his calculations of the latitude of the Obser-

vatory from his observations, according to the manner above explained; in which he first states it at $51^{\circ} 28' 38''$, and finally more correctly in these words. "The apparent zenith distance of the equator, by the mean of 20 observations in 1746-47, $51^{\circ} 27' 28''$. The mean apparent distance of the pole, by the observations made between 1750-52, $38^{\circ} 30' 35''$. Sum $89^{\circ} 58' 3''$. Sum of refractions $1' 57''$. Polar refraction $0' 45''\frac{1}{2}$. Equatorial refraction $1' 11''\frac{1}{2}$. Latitude $51^{\circ} 28' 39''\frac{1}{2}$. Co-latitude $38^{\circ} 31' 20''\frac{1}{2}$."

The latitude of the Observatory being thus settled, as well as the quantity of refractions for all stars passing the meridian between the pole and the equator, Dr. BRADLEY readily inferred from his observations the true distance of all such stars from the north pole, which, compared with their zenith distances observed below the pole, gave the refractions at those lower altitudes. Finally, by comparing the refractions together observed in extreme degrees of heat and cold, he deduced the law of their variation as affected by heat and cold; and thus at length he inferred his elegant rule for determining the refraction in all circumstances, that it is to $57''$, in the direct compound ratio of the tangent of the apparent zenith distance lessened by 3 times the refraction to the radius, and of the height of the barometer in inches to 29.6 inches, and in the inverse ratio of the degree of height of FAHRENHEIT's thermometer increased by 350 to 400.

But it may be proper to confirm this rule for refractions also from the same manuscript of Dr. BRADLEY, which I before cited for confirming the latitude, by the following passage, which immediately follows the other. "Suppose the mean refraction at $45^{\circ} 3' = 57''$, and $y = 350$; then $y + z : \text{bar.} :: 77'' : \text{refr. at } 45^{\circ} 3'$.

"Rad.

“Rad. : tan.ZD :: 3' : m Rad. : $\overline{\tan.ZD - m}$:: refr. at $45^{\circ} 3'$: r
 “ [the refraction required].” It is easy to see that this rule agrees with the other: for putting $t = 50$, and barometer = 29,6, the first analogy, putting the barometer down in tenths of an inch, is $350 + 50 = 400 : 296 :: 77'' : 56'',98$ for the refraction at $45^{\circ} 3'$, or $57''$ within $\frac{1}{100}$ th of a second. The second analogy serves to give the treble refraction nearly, called m . Whence it is evident, the last analogy coincides with the rule above given.

This valuable rule was first communicated by myself to the public in vol. LIV. of the Philosophical Transactions, p. 265. and in p. 49. and 129. of the first edition of Tables requisite to be used with the Nautical Almanac, together with a Table of the mean Refractions deduced from it, with the first Nautical Almanac, that of 1767, published by order of the Commissioners of Longitude in 1766; and again, at page the 5th of the Explanation and Use of the Astronomical Tables, annexed to the first volume of my Observations made at the Royal Observatory from 1765 to 1774, published by order of the President and Council of the Royal Society, with two tables in that work, containing the mean refractions and decimal multipliers for reducing them to any given temperature of the air indicated by the barometer and thermometer. The words in page the 5th of the said preface are as follows. “The astronomical refractions and latitude of the Observatory were settled with the greatest accuracy by Dr. BRADLEY, from his observations of the circumpolar stars, with the brass mural quadrant, during the three years that it was turned to the north, and of the sun and stars in the subsequent years after it was removed to point to the south. The following elegant rule “ was

" was the result of his observations, that the refraction at any
 " altitude is to 57 seconds, in the direct compound ratio of the
 " tangent of the apparent zenith distance lessened by 3 times
 " the refraction to the radius, and of the altitude of the baro-
 " meter in inches to 29,6 inches, and in the reciprocal ratio
 " of the height of FAHRENHEIT's thermometer increased by
 " the number 350, to the number 400. Tables XXII. and
 " XXIII. were adapted to this rule; the first containing the
 " mean refractions answering to 29,6 inches height of the
 " barometer and 50 degrees height of the thermometer; and
 " the second table containing decimals for multiplying the
 " mean refraction in order to find the correction, which applied
 " to it will give the actual refraction, the same as would have
 " been produced by the rule with somewhat more trouble.
 " Dr. BRADLEY supposed the horizontal parallax of the sun
 " $10\frac{1}{2}$ seconds, in the calculations from which he inferred the
 " refractions; and I have been informed, that he determined
 " the latitude of the Observatory $51^{\circ} 28' 39''\frac{1}{4}$. But, had he
 " made use of the true parallax $8''8$ or $8''\frac{1}{4}$, as found by the
 " two late transits of Venus over the sun, he would have made
 " the refraction at the altitude of 45° to be $56''\frac{1}{2}$ instead of
 " $57''$, and the latitude of the Observatory exactly $51^{\circ} 28' 40''$
 " instead of $51^{\circ} 28' 39''\frac{1}{4}$. But his rule for refractions can-
 " not be corrected for all altitudes, without examining his ob-
 " servations of refractions made at various times."

On comparing this extract with M. CASSINI's Memoir, I cannot but express my surprise, that he should not have adverted to a passage containing so direct an application to the grounds of his Memorial, in a publication of such notoriety, and of so old a date as 1776; had he done so, I cannot but think, he would never have hazarded such an opinion as that advanced by him

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in his Memoir of an uncertainty of $15''$ in the latitude of Greenwich; but he might have been induced to believe, that the latitude of this place had been well determined.

For further confirmation of the certainty of the astronomical refractions, and latitude of the Observatory, as settled by Dr. BRADLEY, it may be proper to add, that the Greenwich brass mural quadrant underwent a trial, which all astronomical instruments ought to be submitted to, but which very few ever have been, on account of the difficulty and nicety of the operation, namely, an examination of the total arc; when it was found by Dr. BRADLEY to be an accurate quadrant, the arc appearing at one trial to differ only a fraction of a second from 90° , and another time, after an interval of above six years, to be a perfect quadrant. See p. 24. of BIRD's Method of constructing Mural Quadrants, published by the Board of Longitude in 1768. In like manner he had before examined the total arc of the iron quadrant, first put up by Mr. GRAHAM, for the use of Dr. HALLEY, in the year 1725, by means of a level, and found it to be $16''$ less than a quadrant. See BIRD's Method of constructing Mural Quadrants, p. 7. and Memoires of the Royal Academy of Sciences at Paris, for 1752, p. 424. But this quadrant was, in the year 1753, re-divided by Mr. BIRD, and, in this respect, probably rendered as accurate as the other. See BIRD's Method of constructing Mural Quadrants, p. 24.

Dr. BRADLEY made a curious use of the new set of divisions, soon after they were laid upon the quadrant, to re-examine the error of the total arc laid down originally by Mr. GRAHAM (which by the plumb-line and level he had found to be $16''$ less than a quadrant in 1745) according to the following passage contained in the manuscript before cited.

“ August

“ August 12, 1753, I measured with the screw of my micrometer the difference of the arcs (of $\frac{4}{9}$) as set off by Mr. GRAHAM originally, and by Mr. BIRD when he put on a new set of divisions upon the old quadrant, and I found that Mr. GRAHAM's arc was less than Mr. BIRD's by $\frac{2}{40}$ divisions of my micrometer, which to a radius of 96 inches answers to 10'',6; so that the whole arc of 96 differs from a true quadrant 15'',9, which is the same difference that I formerly found by means of the level, &c.”

Let me further add, that Dr. BRADLEY had informed me, that he had found the same refractions, latitude of the Observatory, and obliquity of the ecliptic, by both quadrants, making a proportionable allowance, in the use of the iron quadrant, for the error of 16'' in the total arc in proportion to the zenith distance of the object before it was new divided.

The Rev. Dr. HORNSBY, F. R. S. Savilian Professor of Astronomy at Oxford, to whose care Dr. BRADLEY's original observations have been committed, in order to their being printed and published, having favoured me with calculations of the latitude of the Royal Observatory from observations of the pole star made with both quadrants, from a manuscript of Dr. BRADLEY, I think it proper to give it a place here, not only as a very curious paper, but also as strongly confirming the latitude of this place before stated.

Transcribed from a loose Paper of Dr. BRADLEY.

“ The mean zenith distances of the pole star above and below the pole, corrected by refraction, aberration, &c. and reduced to January 1751, O. St. as collected from the observations made after the new quadrant was balanced, Nov. 24, 1750.

Number

Number of obser- vations.	Above the pole.	Number of obser- vations.	Below the pole.	to
23	36° 29' 46.66	8	40° 33' 2.95	Jan. 15, 1751
22	45.42	26	3.37	Aug. 27, 1751
23	45.13	27	2.14	May 4, 1752
19	44.63	28	1.67	Nov. 10, 1752
28	45.00	23	1.74	May 31, 1753
9	44.59	8	1.54	July 26, 1753
124	36° 29' 45.24	120	40° 33' 2.24 36° 29' 45.24	

Error of collimation
 $77^{\circ} 2' 47.48$
 $38^{\circ} 31' 23.74$
 1.74

Co-latitude $38^{\circ} 31' 22.0$

$77^{\circ} 1' 15.8$

Collimation	$38^{\circ} 30' 37.9$	Refraction.		
Refraction	$+ 1.4$	$42'' +$	$36^{\circ} 29' 2\frac{1}{2}$	$4^{\circ} 3' 10.8$
	$+ 45.5$	$49 -$	$40^{\circ} 32' 13\frac{1}{2}$	$+ 6.6$
By new quad.	$38^{\circ} 31' 22.0$	$6\frac{1}{2}$	$4^{\circ} 3' 11$	$4^{\circ} 3' 17.4$
	$51^{\circ} 28' 38.0$	Refraction	$+ 6\frac{1}{2}$	

$4^{\circ} 3' 17\frac{1}{2}$
 $2^{\circ} 1' 39$ — the mean distance
 from mean pole Jan. 1751.

The apparent zenith distance of the pole, by the mean of 310 observations, is

$38^{\circ} 30' 36''$ allowing $- 2''$ for the error of the line of collimation.
 Refraction $+ 45\frac{1}{2}$

Latitude $38^{\circ} 31' 21\frac{1}{2}$
 $51^{\circ} 28' 38\frac{1}{2}$

Co-lat. $38^{\circ} 31' 21\frac{1}{2}$ by the new quadrant.

$38^{\circ} 31' 18\frac{1}{2}$ by the old quad. new divisions.

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Apparent

Apparent zenith distances of the pole observed with the iron quadrant.				
1753	Apparent zenith distance of the pole.	Barom.	Thermom.	
			in.	out.
		Inches.	°	°
Sept. 13	38° 30' 40,1	30,10	64	65
23	41,5	29,88	61	60
Oct. 2	42,5	29,67	56	53
5	41,0	30,02	57	57
11	41,1	29,48	61	57
19	40,2	29,82	50	43
31	40,8	29,65	42	34
Nov. 5	42,1	29,69	45	39
16	40,5	29,39	42	35
19	42,2	29,59	40	35
20	40,9	29,84	40	33
24	40,0	30,00	43	35
29	40,0	29,80	38	30
Dec. 3	39,3	30,16	39	33
8	37,4	30,06	35	27
17	41,5	29,50	50	49
30	38,0	30,11	33	22
Mean	38° 30' 40,5	29,81	47	41½
Refraction	+ 46,4			
Col.	— 8,4			
Pol. corr.	38° 31' 18,5			

So far the manuscript.

Thus the latitude by the brass quadrant being $51^{\circ} 28' 38''\frac{1}{4}$, and by the iron quadrant with new divisions $51^{\circ} 28' 41''\frac{1}{4}$, the mean by both quadrants is $51^{\circ} 28' 40''$, or only half a second greater than settled in another manner, according to the manuscript of Dr. BRADLEY in my possession. Also the apparent zenith distance of the pole with the mean refraction $45''\cdot 4$ being $38^{\circ} 30' 36''\cdot 1$ by the brass quadrant, and $38^{\circ} 30' 33''\cdot 1$, by the iron quadrant, the mean by both is 38° .

38° 30' 34'',6, or only $\frac{1}{10}$ ths of a second less than by Dr. BRADLEY's manuscript cited before.

On my promotion to the Royal Observatory in 1765, finding its latitude to have been so accurately settled by Dr. BRADLEY before me, I might have thought myself dispensed from making any particular or very laborious observations for that purpose; however, I confirmed it by my own observations to great nearness, *viz.* within 1 or 2'', at the same time that I was establishing a new catalogue of the principal fixed stars, continually observed here for settling the right ascensions of all other celestial objects with the transit instrument. The result in brief is as follows.

I first settled the relative right ascensions of about 30 of the brightest fixed stars, and lying nearest the equator, by a great number of observations with the transit instrument, referring them to α Aquilæ as the fundamental star, whose right ascension I assumed from Dr. BRADLEY's determination. Hence, by observed transits of the sun and the same stars, in the spring and autumn, when his daily motion in declination was at least 16' or two-thirds of the greatest, I inferred the sun's right ascensions relative to the right ascensions of those stars settled in the manner just mentioned. Also from the sun's observed zenith distances taken with the brass mural quadrant on the same days, and corrected by refraction, parallax, and error of line of collimation, with Dr. BRADLEY's obliquity of the ecliptic, and latitude of the Observatory, I computed the sun's declinations, and thence the right ascensions corresponding to them.

Now, if the assumed right ascension of α Aquilæ, and thence those of the other stars were affected with some small error, as might be supposed, the sun's right ascensions deduced from the observed transits would differ the same way from the

truth at both seasons of the year, viz. by the unknown error of the assumed right ascension of α Aquilæ; but his right ascensions inferred from his observed zenith distances would be affected contrary ways at the two opposite seasons of the year, by the unknown errors in the refractions, parallaxes, latitude of the place, and obliquity of the ecliptic. Hence, the mean of the two corrections of the sun's right ascension, found from the observed declinations about the vernal and autumnal equinox, would be the true correction of the assumed right ascension of α Aquilæ; and the difference of the same corrections would, by an easy calculation, shew how much the computed declinations were too great or too little for the truth, and consequently what the true declinations were, and what the true zenith distance of the sun was, when in the equator, or the latitude of the place, on supposition that Dr. BRADLEY's refractions were truly stated; for any small uncertainty in the obliquity of the ecliptic, as stated by him, could not affect this result, which was deduced equally from observations of the sun in north and south declination, when the same error of the obliquity would affect the sun's right ascensions deduced from the observed declinations contrary ways. I took the sun's parallax from the 24th of my tables annexed to my observations, constructed upon a horizontal parallax $8''.84$, which I had deduced from the observations of the first transit of Venus, that in 1761, and differing insensibly from $8''.4$, which I deduced from the observations of the total durations of the transit between the internal contacts observed at Wardhus and Otaheite in 1769, consequently more correct than the horizontal parallax of $10''.4$ used by Dr. BRADLEY. It is also evident, that the true zenith distance of the equator thus found, diminished by Dr. BRADLEY's mean refraction, will be the apparent

zenith distance of the equator, affected only by the mean refraction.

I shall now give the apparent zenith distance of the equator, and the true latitude of the Observatory, resulting in this manner from my observations of six years, from the autumnal equinox of 1765 to that of 1771, in which I allowed 0",9 for the correction of the error of the line of collimation, additive to the observed zenith distances, as I found from a revision of my calculations of the zenith distances of stars taken with the mural quadrant, compared with the like taken with the zenith sector in 1768.

Years of the observations.	Number of days of observations.	Apparent zenith distance of equator, taking the sun's horizontal parallax 8",8.	True latitude of the Observatory, according to Dr. BRADLEY's refractions and the sun's horizontal parallax 8",8.
Autumnal equinox of 1765 and vernal of 1766	52	51° 27' 28,6	51° 28' 40,1
Autumnal equinox of 1766 and vernal of 1767	38	30,4	41,9
Both equinoxes of 1768	46	32,2	43,7
Both equinoxes of 1769	48	28,7	40,3
Both equinoxes of 1770	20	29,3	40,8
Both equinoxes of 1771	42	29,7	41,3
Mean from six years observations	-	51° 27' 29,8	51° 28' 41,3
Mean found by Dr. BRADLEY, with ☉'s horizontal parallax 10",5	-	51° 27' 28	51° 28' 39,5
But if 1",2 be added to reduce them to the ☉'s horizontal parallax 8",8, Dr. BRADLEY's result will be changed to	-	51° 27' 29,2	51° 28' 40,7
Differing from my determination above only	-	0,6	0,6

In further confirmation of the latitude of the Observatory, I shall now adduce eight years observed zenith distances of the sun in the solstices, being deduced from a number of observations taken at and near the solstices, and corrected for line of collimation, refraction, parallax, and nutation.

Years

Years of observations.	Summer solstitial zenith distance reduced.	N ^o of days of observations.	Winter solstitial zenith distance.	N ^o of days of observations.	Half sum or latitude of the place.	Half difference or obliquity of the ecliptic.
1765	28° 0' 30,2	6	74° 56' 46,1	5	51° 28' 38,2	23° 28' 8,0
1766	33,6	7	48,8	2	41,2	7,6
1767	32,0	6	{ uncertain from an accident. }	—	—	—
1768	29,7	6	44,9	7	37,3	7,6
1769	31,8	8	46,2	5	39,0	7,2
1770	30,7	7	44,1	6	37,4	6,7
1771	32,0	8	42,2	9	37,1	5,1
1772	31,8	12	Line of collimation altered by applying an achromatic object-glass to the telescope.			
Mean Latitude	28° 0' 31,5		74° 56' 45,4		51° 28' 38,4	23° 28' 7,0
	51 28 40					On Jan. 1, 1769.
23 28 8,5 mean obliquity of ecliptic on Jan. 1, 1769.						

This last obliquity $23^{\circ} 28' 8'',5$ is deduced in the manner used by Dr. BRADLEY, and is more to be depended on than the other $23^{\circ} 28' 7'',0$ deduced from both solstices, on account of the less certainty of the lower refractions, from which, however, it only differs a second and a half.

Thus all the observations of the sun and circumpolar stars accord to $1''$ or $2''$ with the latitude of the Observatory settled by Dr. BRADLEY, making use of his refractions.

I shall now determine the latitude independent of Dr. BRADLEY's refractions, and infer the higher refractions at the same time, from a comparison of my observations of the apparent zenith distance of the equator before set down with Dr. BRADLEY's observations of the apparent zenith distance of the pole, both taken with the same excellent brass mural quadrant, in the same manner as Dr. BRADLEY deduced them from

from the apparent zenith distance of the equator observed with the iron quadrant compared with the apparent zenith distance of the pole observed with the brass quadrant, according to the extract from the manuscript in my possession before cited.

The mean apparent zenith distance of the equator, by my observations of six years from 1765 to 1771, was related before $51^{\circ} 27' 29'',8$. The mean apparent zenith distance of the pole was found by Dr. BRADLEY from 1750 to 1752 to be $38^{\circ} 30' 35''$. Their sum $89^{\circ} 58' 4'',8$ taken from 90° leaves $1' 55'',2$, the sum of the refractions at the two zenith distances. Saying then, as $1' 56'',7$ the sum of the refractions by Dr. BRADLEY's rule, to $1' 55'',2$ the sum by observation, so are $1' 11'',4$ and $45'',3$ the respective refractions at the two apparent zenith distances of the equator and pole by Dr. BRADLEY's rule, to $1' 10'',5$ and $44'',7$ the two refractions at those zenith distances, which added to them give the co-latitude $38^{\circ} 31' 19'',7$, and the latitude $51^{\circ} 28' 40'',3$. And as $1' 56'',7 : 1' 55'',8 ::$ so is $57''$ the refraction at the apparent zenith distance $45^{\circ} 5'$ by Dr. BRADLEY : $56'',27$ the true refraction at that zenith distance, or not half a second differing from Dr. BRADLEY's, but more to be depended on as deduced from observations made with the brass quadrant only, and calculated from a parallax of the sun nearer to the truth.

But if the apparent zenith distance of the pole be made use of, resulting from a mean of 310 observations made with the brass quadrant, according to Dr. BRADLEY's manuscript, communicated by Dr. HORNSBY, from the whole of his observations from 1750 to 1753, *viz.* $38^{\circ} 30' 36''$, the sum of this and $51^{\circ} 27' 29'',8$, the apparent zenith distance of the equator found by myself with the same instrument, or $89^{\circ} 58' 5'',8$ taken from 90° leaves $1' 54'',2$, the sum of the two refractions

at

at the pole and equator. Whence the refraction at the pole will be found in like manner as before $44''$, 3 , and that at the equator $1' 9''$, 9 , and the latitude $51^{\circ} 28' 39''$, 7 , and the refraction at the apparent zenith distance of $45^{\circ} 3' = 55''$, 8 , which is $1''$, 2 less than Dr. BRADLEY's determination, and $1''$, 2 greater than deduced from Mr. HAWKSBEЕ's experiment of the refraction of the air hereafter cited. It will be shewn in the sequel, that the latitude thus found does not at all depend on the truth of the total arc, but only supposes the instrument proportionally divided at the points answering to the pole and the equator.

From the whole then I conclude, that the latitude of the Royal Observatory at Greenwich is firmly established from Dr. BRADLEY's observations and my own at $51^{\circ} 28' 40''$, probably without the error of a single second.

Let us now inquire into the latitude of the Royal Observatory at Paris. M. LE MONNIER, in the Memoires of the Royal Academy of Sciences for 1738, and in his *Histoire Celeste*, has examined into the latitude of the Royal Observatory at Paris, resulting from the observations of the principal French astronomers, and assuming the refraction at the height of the pole at Paris to be $50''$, which is $2''$ less than DOMINICO CASSINI's table gives, and the same which Dr. BRADLEY's rule gives, he finds the latitude of their Royal Observatory as follows:

From the observations of M. PICARD	-	$48^{\circ} 50' 10''$
_____ of M. DE LA HIRE	-	$48 50 12$
_____ le Chev. DE LOUVILLE		$48 50 8$
_____ M. Maraldi	-	$48 50 14$

His

His own observations in 1738, after examining and making an allowance for the error of the total arc of his quadrant - - - - 48 50 14

His further observations in 1740, making allowance for the error of the total arc of his quadrant, and considering the effect of the state of the air indicated by the thermometer upon the refractions 48 50 15

In the Memoires of the Royal Academy of Sciences for 1744, M. CASSINI DE THURY (the author of the memoir) finds from his own observations, with the same refractions - - - - 48 50 12

In the Memoires of 1755, the Abbé DE LA CAILLE, from a nice and accurate calculation of his observations made at the College of Mazarine, at Paris, and the Cape of Good Hope, deduces new tables of refraction suitable to each place, and states their respective latitudes, and thence that of the Royal Observatory at Paris - - - - 48 50 14

Hence the ancient observations of M. PICARD, M. DE LA HIRE, and the Chevalier DE LOUVILLE give 48 50 10

The modern and more accurate observations of M. MARALDI, M. LE MONNIER, M. CASSINI DE THURY, and the Abbé DE LA CAILLE, give - 48 50 14; which is now generally made use of by the French astronomers as the true latitude of their Royal Observatory; and from the near agreement of so many diligent observers and able astronomers cannot be supposed to differ above 2 or 3'' from the truth. The difference of this and $51^{\circ} 28' 40''$, the latitude of the Royal Observatory at Greenwich above stated, is $2^{\circ} 38' 26''$, the true difference of latitude of the two Observatories, which,

from what has been said of the observations on which the respective latitudes were founded, cannot be supposed to differ above 3 or 4'' from the truth. What then becomes of the uncertainty of 15'' supposed by the late M. CASSINI?

The same difference of latitude I find nearly from a comparison of my own observations of γ and β Draconis, taken with the zenith sector in 1768, with those of the Abbé DE LA CAILLE in 1750 and 1756, given in his *Fundamenta Astronomiæ*, after making the proper allowances for aberration, precession, and nutation, and correcting my observations by Dr. BRADLEY's refraction, and the Abbé DE LA CAILLE's by his table, and making allowance for the distance of the Abbé DE LA CAILLE's Observatory from their Royal Observatory; viz. $2^{\circ} 38' 25'',4$ from γ Draconis, and $2^{\circ} 38' 26'',1$ from β Draconis; the mean being $2^{\circ} 38' 25'',7$, differing only $0'',3$ from that stated above; but from Dr. BRADLEY's observations $2^{\circ} 38' 24'',9$, and $2^{\circ} 38' 27'',2$, mean $2^{\circ} 38' 26'',0$. It is too well known to astronomers to need my pointing out, that the best method of determining the difference of latitude of places, differing but little in latitude, is by such differences of zenith distances of stars passing near the zeniths, as the two above cited, observed at both places, in the same manner as the amplitude of the celestial arc is observed for finding the length of a degree of the meridian by comparison with geometrical measures.

The question now will be, upon what foundation was the late M. CASSINI's supposition of an uncertainty of 15'' in the latitude of Greenwich built? This appears evidently to have been upon a passage in the Abbé DE LA CAILLE's researches into the astronomical refractions and latitude of Paris, contained in the Memoires of the Royal Academy of Sciences for

1755,

1755, p. 578, 579, where M. DE LA CAILLE takes the differences of zenith distances of 14 stars observed by Dr. BRADLEY (in correspondence to the same observed by himself at the Cape of Good Hope, for determining the moon's parallax in declination) published in the Memoires of the Royal Academy of Sciences for 1752, and the same observed by himself at Paris, after his return from the Cape, and correcting them for the difference of the refractions at the respective zenith distances, according to his own table of refractions, and the known apparent motions of the stars, finds the mean $2^{\circ} 37' 23''.9$, which added to $48^{\circ} 51' 29''.3$, his latitude at the College of Mazarine, gave him $51^{\circ} 28' 53''.2$ for the latitude of Greenwich, exceeding Dr. BRADLEY's latitude by 13 or 14''.

Now the legitimacy of this conclusion depends upon a supposition that both instruments measured the true angle, or that their total arcs were justly laid off, and that the Abbé DE LA CAILLE's table of refractions is just. The first indeed has been proved with respect to Dr. BRADLEY's quadrant, but never has been attempted with respect to the Abbé DE LA CAILLE's sextant; for the examination which the Abbé made of his instrument by parts for every $7^{\circ}\frac{1}{2}$ (see Memoires of the Royal Academy of Sciences for 1751 p. 405.), could not determine the error of the whole arc, as the difference from the truth might be insensible upon such small arcs, and the examination seems to have been intended to find the differences of these small arcs from one another rather than from the true arc which they represent. We may therefore be allowed to doubt of the truth of this circumstance. This doubt will be further strengthened by several particulars which I shall adduce.

1. The apparent altitude of the pole at the Royal Observatory $48^{\circ} 51' 12''$, resulting from the Abbé DE LA CAILLE's

observations, exceeds $48^{\circ} 51' 4''$ the mean of the observations of Mess. MARALDI, LE MONNIER, and CASSINI DE THURY, by $8''$, and at the same time his refractions for that altitude exceed what they adopt by the same quantity. 2. His refractions are greater than all other tables give, DOMINICO CASSINI's, FLAMSTEED's, NEWTON's, BRADLEY's, MAYER's, SIMPSON's, and Lord MACCLESFIELD's. The latter I have by me in a manuscript of Dr. BRADLEY, being what he used to correct his observations by, before he had been enabled to determine the refractions with the new mural arc. They were deduced from a brass quadrant of 5-feet radius made by Mr. Sisson, still remaining in the Observatory at Sherburn-Castle, and are the more to be esteemed because the divisions of the instrument had been submitted to the *strictest* *re-examination*, whereby, in the opinion of Dr. BRADLEY, it was probably rendered *as perfect in its kind as any extant, or as human skill could at that time produce*. See Dr. BRADLEY's Letter to Lord MACCLESFIELD, Phil. Transf. vol. XLV. p. 5. The refractions in this table are less than Dr. BRADLEY's by $2''$, 4 at the altitude of 45° , and $4''$ at the altitude of 20° . MAYER's refractions agree almost exactly with Dr. BRADLEY's, and are entitled to much weight, having been determined by a 6-feet mural arc constructed by Mr. BIRD. 3. The refractions were found by the French Academicians at the polar circle, according to M. MAUPERTUIS's Book on the Figure of the Earth, to agree nearly with DOMINICO CASSINI's table. Hence it may be inferred, that the refractions in a warmer climate, as France, should be less than according to the same table, and therefore much less than according to M. DE LA CAILLE's, and approaching to Dr. BRADLEY's, which are a little less than M. CASSINI's. 4. M. LE MONNIER, after his return from the polar circle, with a quadrant examined at the zenith and horizon, and after making allowance for the error thereby inferred in the total arc, observed

served a great many refractions of stars under the pole, with the state of the thermometer, and sometimes of the barometer also, as recorded in his *Histoire Céleste*. These I calculated formerly, and found the refractions observed in very hot and very cold weather, compared together, to follow the same rate of increase and decrease, according to the changes of temperature, as Dr. BRADLEY has assigned; and, reducing the observed refractions to the mean temperature, I found them agree nearly with Dr. BRADLEY's. 5. The refractive power of the air about its mean temperature was carefully observed by Mr. HAWKSBEE, as related in his *Physico-Mechanical Experiments*, and the ratio of the sine of incidence to that of refraction out of air into a vacuum found to be as 999736 to 1000000. Hence the astronomical refraction at the altitude of 45° should be $54''.6$, only $2''.4$ less than Dr. BRADLEY's, and $2''$ less than the same when his higher refractions are now calculated with the true parallax of the sun, and $1''.2$ less than I have before shewn to result from my observations of the apparent zenith distance of the equator compared with Dr. BRADLEY's of the apparent zenith distance of the pole, both taken with the same brass mural quadrant, but $1.2''$ less than the Abbé DE LA CAILLE's.

From all these facts, I think, I may be allowed to conclude, that the Abbé DE LA CAILLE's refractions are not just, but considerably too large; and, consequently, as there can be no doubt of the care or diligence used by this astronomer in his observations and calculations, that the total arc of his instrument is too large for the radius, and, as I shall shew presently, gives the measures of the zenith distances too small.

But it may be asked, are then all the observations of this great astronomer, with their results, the fruit of so much labour and pains, to be considered as uncertain or lowered in their value in proportion to the error of his instrument? I am happy to answer,

answer, that the very ingenious method which he used of getting his refractions from the comparison of the sum of the apparent altitudes of the poles at Paris and the Cape with the sum of the apparent zenith distances of stars passing the meridian between the two places, has fortunately, without his being aware of it, given him the refractions affected with the error of the arc of the instrument, and consequently proper for correcting his observations; for, if the instrument be supposed ill divided, any error in the divisions will naturally be thrown upon the refractions; and, if the total arc is too large for the radius, the stars will appear to approach the zenith by the error of the divisions as well as the refractions, and the refractions in the table will come out too large, but still suitable to the instrument, because a correction is necessary to be added to the observed zenith distance, on account of the error of the instrument, as well as of the true refractions, and the table deduced from the instrument gives the sum of the two corrections together, without determining them separately.

Hence his table of refractions, though well adapted to his instrument, may be very unfit to be applied to any other. His latitudes of his observatories and his declinations of the stars will not lose any of their certainty, at least within the limits of the zenith distances measured by his sector, *viz.* 60° . And this accounts for a circumstance, at first sight rather extraordinary, that his declinations of stars should agree so nearly (generally within $5''$ of Dr. BRADLEY's, as Dr. BRADLEY himself remarked) though his refractions made use of were so very different.

Having now shewn, that the Abbé DE LA CAILLE's refractions are too great, and only fit to be applied to his own instrument, it will be easy, by a just calculation, to reconcile the
before

before mentioned zenith distances of 14 stars, observed at Greenwich by Dr. BRADLEY and by the Abbé DE LA CAILLE at the College of Mazarine at Paris, with the established latitudes of the two Observatories, nearly; in doing which I shall claim the same right to correct Dr. BRADLEY's observations by his table of refractions, as I have allowed the Abbé DE LA CAILLE to be intitled to correct his observations by his table of refractions; which, I think, will be allowed me, after what I have said of the manner in which the Greenwich refractions were deduced and the instruments made use of. The difference of latitude of the College of Mazarine and the Royal Observatory at Greenwich will then come out by the several stars, as follows; $2^{\circ} 37' 12'',7$, $16'',0$, $13'',8$, $13'',7$, $18'',4$, $17'',7$, $19'',7$, $23'',2$, $17'',7$, $17'',0$, $13'',9$, $12'',4$, $5'',6$, $11'',9$. The mean is $2^{\circ} 37' 15'',2$ (or $8'',7$ less than the Abbé DE LA CAILLE's result in his method of calculation, which I have shewn to be inadmissible) and added to $48^{\circ} 51' 29'',3$, the latitude of the Abbé DE LA CAILLE's Observatory gives $51^{\circ} 28' 44'',5$ for the latitude of the Royal Observatory at Greenwich, only $4''\frac{1}{2}$ more than established by Dr. BRADLEY's observations and my own; a sufficient agreement, especially considering that many of the stars were at great distances from the zenith, and that no account has been made of the temperature of the air at the times of the observations. The proper method, however, of settling the difference of latitude of two Observatories is by stars near the zenith, as I observed before; and the difference of the latitudes of the two Observatories of the College of Mazarine and Greenwich, by the Abbé DE LA CAILLE's observations of β and γ Draconis compared with mine, was $2^{\circ} 37' 10'',4$, and compared with Dr. BRADLEY's $2^{\circ} 37' 10'',7$, the first of which, added to the Abbé DE LA CAILLE's latitude, gives.

51° 28' 39'',7, and the other 51° 28' 40'', for the latitude of the Royal Observatory at Greenwich, exactly agreeing with that deduced immediately from the observations made at this place.

The same result nearly follows from M. CASSINI DE THURY's own observations of the zenith distance of the sun at the summer solstice of 1755, contained in the Memoires of the Royal Academy of Sciences for that year, compared with Dr. BRADLEY's, which latter was communicated to me by the late JOHN HOWE, Esq.; for, by M. CASSINI's observations, the solstitial altitude of the sun's upper limb, corrected by the difference of refraction and parallax, according to DOMINICO CASSINI's table, which happens to agree with the same difference by my tables at this height, was 64° 53' 36'', from which 15' 47'' being subtracted for the semi-diameter of the sun according to MAYER's tables, there remains 64° 37' 49'', the true altitude of the sun's center, and consequently the sun's true zenith distance 25° 22' 11''. But the same was found from Dr. BRADLEY's observations, by my tables of refractions and the sun's parallax, 28° 0' 32'',8. The difference 2° 38' 21'',8 or 2° 38' 22'' is the difference of latitude of the two Observatories, which added to 48° 50' 14'', the latitude of the Royal Observatory at Paris, gives 51° 28' 36'' for the latitude of the Royal Observatory at Greenwich, or only 4'' less than before stated from the Greenwich observations, the difference lying the contrary way to that which the Abbé DE LA CAILLE carried the latitude of Greenwich, by improperly applying his own table of refractions to the Greenwich observations as well as to his own.

The Abbé DE LA CAILLE having, in the sequel of his memoir, inferred the difference of latitude of Gottingen and the College of Mazarine from 22 stars observed by M. MAYER
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with a 6-feet mural quadrant of BIRD's construction, correspondent to the same observed by himself, I shall make some remarks on this comparison, because it appears to me to afford a strong argument to shew that the Abbé DE LA CAILLE's refractions are too great; and that MAYER's, which agree with Dr. BRADLEY's, are just. The Abbé, after correcting the zenith distances of 22 stars observed at both places by his own table of refractions, finds the difference of latitude of Gottingen and Paris, by a mean, to be $2^{\circ} 40' 35''$,₁, which added to $48^{\circ} 51' 29''$,₃, the latitude of the College of Mazarine, gives him the latitude of M. MAYER's Observatory $51^{\circ} 32' 4''$,₄. He adds, that some observations of the pole star sent to him by M. MAYER would give the latitude of Gottingen $19''$ less than he has established it, as just mentioned. Now I find, that if M. MAYER's observations of the pole star, as well as of the stars to the south of the zenith, be corrected by M. MAYER's table of refractions, and the Abbé DE LA CAILLE's observations by *his* table of refractions, the latitude resulting from M. MAYER's observations of the pole star will agree to $2''$ with that resulting from the difference of latitude by the stars to the south; for subtracting $19''$ from $51^{\circ} 32' 4''$,₄, the latitude which the Abbé DE LA CAILLE has assigned to Gottingen in the manner above-mentioned, there remains $51^{\circ} 31' 45''$,₄ the latitude which he found by the pole star; to which adding $52''$,₈, the refraction at the mean height of the pole star according to the Abbé DE LA CAILLE, the sum $51^{\circ} 32' 38''$,₂ must be the apparent height of the pole by Mr. MAYER's observations, which diminished by $45''$,₆, M. MAYER's refraction, gives the true latitude of M. MAYER's Observatory $51^{\circ} 31' 52''$,₆. But by the difference of the Abbé DE LA CAILLE's and M. MAYER's zenith distances of the 22 stars to

the south, corrected each by their own table of refractions, I find the difference of latitude $2^{\circ} 40' 32'', 0$, $28'', 9$, $25'', 1$, $27'', 9$, $22'', 6$, $28'', 3$, $32'', 0$, $26'', 7$, $27'', 2$, $24'', 2$, $25'', 4$, $31'', 3$, $23'', 2$, $25'', 6$, $28'', 9$, $18'', 4$, $16'', 7$, $22'', 0$, $23'', 8$, $21'', 1$, $27'', 3$, $27'', 3$, the mean of which is $2^{\circ} 40' 25'', 6$, which, added to $48^{\circ} 51' 29'', 3$, the latitude of the College of Mazarine, gives the latitude of Gottingen $51^{\circ} 31' 54'', 9$, or only $2'', 3$ more than I have deduced above from M. MAYER's observations of the pole star rightly corrected, and only $0'', 9$ less than is set down in M. MAYER's tables, which he expressly says, p. 48. of the precepts to his solar and lunar tables, published by myself for the Commissioners of Longitude in 1770, was deduced from his own observations.

I have before, when I shewed the Abbé DE LA CAILLE's refractions to be considerably too great, at the same time vindicated them as fit for his instrument, because he deduced them in a manner which gave him the apparent elevation of objects above their true place by the sum of refraction and the error of his instrument, if his instrument measured the zenith distances too small, as I had concluded it did. The like remark may be applied to Dr. BRADLEY's table; for his refractions at the pole and equator, having been determined with one and the same quadrant, at one time turned to the north to observe the apparent zenith distance of the pole by means of the polar star and other circumpolar stars, and afterwards to the south to observe the apparent zenith distance of the equator, in the manner before explained, must necessarily be the true refractions, if the instrument measured the true angle; and the sum or difference of the true refractions and the errors of the instrument for these zenith distances, in case the instrument did not measure the true angle; and therefore equally proper to correct his observations, whether the total arc was just or not.

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Moreover,

Moreover, from the two refractions thus found at the equator and pole, the refractions of the circumpolar stars at their passing the meridian above the pole were computed by Dr. BRADLEY, from the hypothesis that the refractions at considerable altitudes are as the tangents of the zenith distances; which rule is pretty accurately true with respect to the real refractions, and would vary but little from the truth for the apparent refractions, which would be the sum or difference of the true refractions and the errors of the arc, in case the total arc erred from the truth by a very small quantity, not exceeding 10 or at most 20 seconds. The observed zenith distances of the stars above the pole being corrected by the refractions thus computed, and subtracted from the known co-latitude, gave their true distances from the north pole, which added to the co-latitude gave their true zenith distances under the pole; and this diminished by the observed zenith distance would give the refraction under the pole, or the sum or difference of the refractions and the errors of the instrument belonging to their respective zenith distances; and thus his whole table would exhibit the sum or difference of the true refraction and error of the instrument. Hence the latitude of Greenwich established by Dr. BRADLEY, with his quadrant, as well as the latitudes of the Observatories at the College of Mazarine and the Cape of Good Hope, settled by the Abbé DE LA CAILLE with his sextant, and the declinations of the stars and the obliquity of the ecliptic found by both will be very near the truth, independent of the justness of the total arcs, although their respective refractions may be suitable only to their own particular instruments. But, for the reasons before given, I apprehend, the Abbé DE LA CAILLE's refractions to be much too large, and Dr. BRADLEY's to be very near the truth.

I shall now close my enquiry into the latitudes of Greenwich and Paris, and Dr. BRADLEY's and the Abbé DE LA CAILLE's refractions, by a remark naturally arising from my comparison of, and endeavours to reconcile, their observations, which I desire to submit to the consideration of astronomers, it not having, that I know of, been made before; that a table of refractions should be made for every vertical instrument from observations made with itself turned alternately north and south; and that the table, so made, applied to observations made with it, will give the true zenith distances, whether the total arc of the instrument be accurately just, or affected with a small error, or however unequally it be divided below the pole, provided the divisions are equal between themselves in the part of the instrument lying between the equator, the zenith, and the pole.

It remains to give some account of the longitude of Greenwich, or rather of the difference of meridians of Greenwich and Paris, in reply to the late M. CASSINI's doubts on the subject. This had been settled by Dr. BRADLEY at $9' 20''$, as he informed me himself, and that he had deduced it from eclipses of Jupiter's first satellite observed at both places, and that he had found it come out the same both from the immersions and emersions. This quantity had been inserted in the table of latitudes and longitudes of places, prefixed to Dr. HALLEY's tables, on the authority of Dr. BRADLEY, so long ago as the year 1749, the date of the publication of those tables, and was generally admitted by astronomers till the year 1763, when the late Mr. JAMES SHORT, F. R. S. computed it from the four transits of Mercury over the sun in 1723, 1736, 1743, and 1753, observed at Paris, London, and Greenwich, to be $9' 16''$. See Philosophical Transactions, Vol. LIII. p. 158. In the

the year 1776, I requested the late Mr. WARGENTIN, the learned Secretary of the Royal Academy of Sciences at Stockholm, and Author of the improved Tables for computing the Eclipses of Jupiter's Satellites, who collected observations of them from the principal Observatories of Europe, in order to the further improvement of the tables, to inform me what difference of meridians of Greenwich and Paris resulted from my last ten years observations of the eclipses of the first satellite of Jupiter compared with those made by M. MESSIER at Paris. In the answer which he favoured me with, inserted in the Philosophical Transactions, Vol. LXVII. p. 162. he set down the result of the comparisons of eight corresponding immersions and nine emersions observed on both parts, by myself and M. MESSIER, from which he deduced the difference of meridians of the Royal Observatories of Greenwich and Paris $9' 35''$. By two corresponding immersions and nine corresponding emersions, observed at both Royal Observatories, he found $9' 21''$. From the observations made between 1761 and 1764 he found $9' 28''$. By the observations made before 1700, $9' 21''$. And, from a comparison of mine and the Parisian observations, with the intermediate help of his own made at Stockholm, $9' 26''$: and from the whole he inferred the difference of meridians to be $9' 25''$.

Twelve years having elapsed since Mr. WARGENTIN's comparison, I was desirous to see what would result from the further observations made during that time, and applied to the Comte CASSINI, the respectable heir of the late M. CASSINI DE THURY and his successor at the Royal Observatory, and to the celebrated M. MESSIER, to favour me with such of their observations of the eclipses of the first satellite of Jupiter as had been made correspondent to mine. These they immediately sent me in the

the most obliging manner, by which I am enabled to make further inferences concerning the difference of our meridians, as exhibited in the two following tables; in reference to which it is to be understood, that all the observations at Greenwich were made with a 46-inch achromatic telescope of 3,6 inches aperture, except a few otherwise noted as observed with a 6-feet Newtonian reflector, whose aperture is 9,4 inches; and once with a 2-feet Gregorian reflector, whose aperture is 4,5 inches; and once with an 18-inch Gregorian reflector, with an aperture of 4,4 inches, furnished with new metals of Mr. EDWARDS's brilliant composition, described in the Appendix to the Nautical Almanac of the present year, which reflects as much of the incident light as an achromatic telescope transmits; and that in making out the columns, intitled difference of meridians corrected, I have subtracted 7'' from the immersions and added as much to the emersions observed with the 6-feet reflector, and added 13'' to the immersions and subtracted as much from the emersions observed with the 2-feet reflector, to reduce them to what they should have been probably observed at with the 46-inch achromatic telescope, and added 5'' to the time of the emersion observed on Sept. 5, 1784 at the Royal Observatory at Paris, with a 5-feet reflector of DOLLOND, to reduce it to the 3½ feet achromatic telescope.

Difference

and Longitude of the Royal Observatory at Greenwich. 183

Difference of meridians of the Royal Observatories at Greenwich and Paris, by observations of eclipses of Jupiter's first satellite, observed at both places.

By Immersions.			Circumstances of the observations at Greenwich.	Circumstances of the observations at the Royal Observatory at Paris.
	Difference of meridians.	Diff. of meridians corrected.		
1779, Jan. 11	9 22	9 29	6 F.	Limbs undulating.
18	9 38	9 45	6 F.; air a little hazy.	Air very clear.
1780, Jan. 14	9 5	9 5	Air very clear.	
Mar. 18	9 16	9 16	Air very clear.	Air hazy.
25	9 8	9 8	— —	Air a little hazy.
1785, Oct. 1	9 11	9 11	{ In contact with Jupiter's body.	A little hazy.
Mean of 6 imm.	9 17	9 19		
By Emersions.			Circumstances of the observations at Greenwich.	Circumstances of the observations at the Royal Observatory at Paris.
	Difference of meridians.	Diff. of meridians corrected.		
1773, Nov. 1	9 10	9 3	6 F. Air a little hazy.	
1775, Feb. 15	10 44	10 37	6 F. Twilight.	Air very clear.
Mar. 17	9 43	9 36	6 F. Air a little hazy.	Air very clear.
1778, Mar. 13	10 46	10 46	—Bright moonshine.	
Apr. 12	9 46	9 46	Air hazy.	
1779, Mar. 30	10 43	10 43	— —	Air hazy.
Apr. 1	9 56	10 9	{ 2 F. reflector. Air very clear.	
1781, May 24	9 49	9 49	Air very clear.	
31	9 8	9 8	— —	Air very clear.
1782, Aug. 29	9 27	9 27	Air very clear.	
1783, Aug. 2	8 54	8 54	Air very clear.	{ The sat. very near Jupiter's disk.
Oct. 26	9 45	9 45	{ Air very clear, but Jupiter low.	{ Limbs undulating much.
1784, Sept. 5	9 4	9 9	Air very clear.	{ 5-foot reflector by DOLLOND, magnifying 450 times.
1785, Nov. 9	9 26	9 26	{ 18 inch reflector, new metals.	
16	9 45	9 45	— — —	Hazy.
Mean of 15 emer.	9 44.4	9 42		
Mean of 6 imm.	9 17	9 19		
Mean of both means	9 31	9 30½		

Before

Before the 24th of May, 1781, a 3 $\frac{1}{2}$ -feet achromatic telescope of DOLLOND, of 42 lines aperture, that was but an indifferent one, was made use of at the Royal Observatory at Paris. From that time a very good one was employed of the same size and aperture.

Difference of meridians of the Royal Observatory of Greenwich and the Hôtel de Clugny at Paris, 2'' of time East of the Royal Observatory, deduced from observations of eclipses of Jupiter's first satellite observed at both places.

	By Immersions.		Circumstances of the observations at Greenwich.	Circumstances of the observations at Hôtel de Clugny.
	Difference of meridians.	Diff. of meridians corrected.		
1775, July 15	9 55	9 55	- -	Air very clear.
Aug. 7	9 6	9 13	6 F.	Air very clear.
1776, Sept. 10	9 22	9 22	Jupiter a little hazy.	
1777, Sept. 6	9 33	9 33	- -	Air very clear.
Nov. 7	9 23	9 23	Air hazy.	Air very clear.
1779, Jan. 11	10 18	10 25	6 F.	Air very clear.
18	9 23	9 30	6 F. air a little hazy.	Air very clear.
Dec. 22	8 10	8 10	Air very clear.	A little hazy.
1780, Jan. 7	9 39	9 39	Air very clear.	Air very clear.
14	9 40	9 40	Air very clear.	Air very clear.
Mar. 18	9 2	9 2	Air very clear.	Air very clear.
25	9 24	9 24		
1781, Feb. 26	8 48	8 48	J's limb undulates.	
Mar. 5	8 59	8 59	- -	Air very clear.
Apr. 13	9 16	9 16	- -	Jupiter ill defined.
1783, July 8	9 58	9 58	- -	Air very clear.
1786, Sept. 4	9 19	9 19	Air very clear.	
Dec. 30	9 31	9 31	Air very clear.	Very hazy.
Mean of 18 imm.	9 22.5	9 23.7		

By Emerfions.		Diff. of meridians corrected.	Circumstances of the observations at Greenwich.	Circumstances of the observations at the Hôtel de Clugny.
	Difference of meridians.			
1775, Feb. 15	9' 31"	9' 24"	6 F. Twilight.	Air very clear.
22	9' 1	8' 54"	6 F.	Air very clear.
Mar. 17	9' 13	9' 6	6 F. Air a little hazy.	Hazy.
1776, Jan. 26	9' 15	9' 15	— —	—
1777, Feb. 4	8' 51	8' 51	Air very clear.	Air very clear.
Mar. 24	9' 13	9' 13	Air very clear.	Air very clear.
31	9' 22	9' 22	Air very clear.	—
1778, Feb. 25	8' 48	8' 48	— —	Air hazy.
Mar. 13	9' 15	9' 15	— —	A little hazy.
Apr. 5	9' 17	9' 17	Air hazy.	Air very clear.
12	10' 11	10' 11	— —	Air very clear.
May 21	9' 29	9' 22	6 F.	Air very clear.
1780, Apr. 19	9' 28	9' 28	Air very clear.	—
May 28	9' 37	9' 37	— —	Air very clear.
1781, May 31	9' 10	9' 10	— —	Air a little hazy.
June 16	9' 25	9' 25	— —	Air a little hazy.
1783, Aug. 2	9' 23	9' 23	Air very clear.	Air very clear.
25	9' 40	9' 40	Air very clear.	—
Oct. 3	9' 30	9' 30	Air very clear.	Air hazy.
1785, Nov. 9	9' 14	9' 14	{ 18-inch reflector, new metals.	—
18	9' 12	9' 12	Air very clear.	—
1786, Jan. 3	9' 58	9' 58	Air a little hazy.	Air hazy.
Mean of 12 emer.	9' 22	9' 20,7	M. MESSIER's achrom. telescope is of 3½ feet focus, 40 lines aperture, with magnifying powers of 70 and 140.	
Mean of 18 imm.	9' 22,5	9' 23,7		
Mean by both Royal Observ. west of Hôtel de Clugny }	9' 22	9' 22		
	— 2	— 2		
Diff. of merid. of the Royal observatories }	9' 20	9' 20		

Hence the difference of meridians of the two Royal Observatories, by the observations made in the Royal Observatories themselves, is 9' 30"; and by the observations made by

M. MESSIER, at the Hôtel de Clugny, and reduced to the Royal Observatory is $9' 20''$. The mean of both results is $9' 25''$. But if greater weight be given to the latter determination than to the former in the ratio of 2 to 1, on account of the series of M. MESSIER's observations being the most complete, the difference of meridians will be $9' 23''$.

M. DU SEJOUR, in the Memoires of the Royal Academy of Sciences for 1771, found $9' 20''$, as well from the beginning as end of the solar eclipse of 1769. M. MECHAIN, the learned editor of the *Connoissance des Temps*, informs me, that from the immersions of Celeno and Maia at the moon's limb, on March 5th last year, he has found by calculation from M. MESSIER's observations compared with mine $9' 19'',9$ and $9' 17'',9$, or by a mean $9' 18'',9$; but, by his own observations compared in like manner, he makes it a little more than $9' 20''$. He regulated his clock by corresponding altitudes; but M. MESSIER corrected his by a transit instrument, which, however, has no meridian mark. For the present, I infer, we may take the difference of meridians $9' 20''$, as being within a very few seconds of the truth, till some more occultations of fixed stars by the moon, already observed, or hereafter to be observed, in favourable circumstances, and carefully calculated, shall enable us to establish it with the last exactness. To collect and calculate such observations I have not leisure at present; but the field of calculation is equally open to the celebrated astronomers of Paris, the observations made at this place being now published annually.

The extensive geometrical operations recommended by the late M. CASSINI DE THURY, and commenced under the direction of Major-general ROY, F. R. S. by his exact measure of a base on Hounslow-Heath, may also, when completed, determine

mine the difference of meridians of Greenwich and Paris to great exactness. But they do not seem to me likely to throw any new light on the difference of latitude of the two Observatories, because the uncertainty we are still under about the true figure and dimensions of the earth, and the irregular attractions arising from the irregular external figure and unequal density of the internal parts of the earth would prevent us from drawing any accurate conclusions, or such as we could confide in, from those geometrical measures, with respect to so large a quantity as $2^{\circ} 38' 26''$ the difference of latitude; and, at all events, it must be less exact, as it is less direct, to determine the difference of latitude of two places from the measured distance of the two parallels compared with the length of a degree in the intermediate latitude, inferred from former measures of degrees, which were themselves determined with the help of astronomical observations, than to infer it from the immediate astronomical observations made at the two observatories, in the manner I have already deduced it.

NEVIL MASKELYNE,
Astronomer Royal.

Greenwich,
February 21, 1787.



XIX. An Account of the Mode proposed to be followed in determining the relative Situation of the Royal Observatories of Greenwich and Paris. By Major-General William Roy, F. R. S. and A. S.

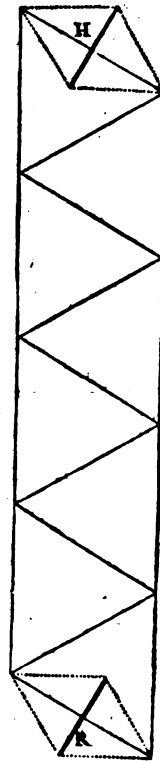
Read February 22, 1787.

TWO years have nearly elapsed since an account of the measurement of a base on Hounslow-Heath was laid before the Royal Society, being the first part of an operation ordered by his Majesty to be executed for the immediate purpose of ascertaining the relative situations of the Royal Observatories of Greenwich and Paris; but whose chief and ultimate object has always been considered of a still more important nature, namely, the laying the foundation of a general survey of the British Islands.

When the operation commenced in 1784, it was not doubted, that in 1786, at latest, we should have been able to have proceeded with the series of triangles from Hounslow-Heath to the neighbourhood of Dover; but the contrivance and construction of an instrument, new of its kind, proposed to be made use of, and more particularly the nicety of its division, whereby it is hoped the angles may be determined to a degree of precision hitherto unexampled, have required much more time than Mr.

RAMSDEN himself at first imagined. Without meaning to disappoint, this ingenious artist was perhaps in the outset too remiss and dilatory, and accidents having happened when the workmanship was already far advanced, which he could not foresee or prevent, the execution has thereby been greatly retarded. However, since the instrument may at present be considered as nearly finished (such parts as yet remain to be perfected being only of the smaller kind), we may fairly conclude, that early in the ensuing summer, or as soon as the weather in this country will permit, the trigonometrical operation may be begun. In this state of things, I have therefore judged that it might be proper to lay before the Society a short sketch of the mode which is proposed to be followed in fulfilling his Majesty's commands, accompanied by a very slight general map of the country, only collected from the common surveys, but still sufficient to shew nearly the disposition of the triangles that will be made use of in forming the junctions between the meridians of the two Observatories. In this business it will be understood, that I mean to adhere to such principles as have been universally received and admitted as just.

In every series of triangles where each angle is to be actually observed with the same instrument, they should, as near as the circumstances will permit, be equilateral: for were it possible to choose the stations in such a manner as that each angle should be exactly 60 degrees, the half number of triangles in the series, multiplied by the length of one side, would, as in the annexed figure, give at once the total distance; not only the sides of
the



the scale or ladder would be perfectly parallel, but the diagonal steps, marking the progress from one extremity to the other, would be alternately so throughout the whole length. The first side is supposed to be found by the measurement of a base H, of about half its length; and the last side to be verified by such another base R at the opposite extremity.

In any particular case, where only two angles of a triangle can be actually observed, these should be as near as possible each 45 degrees. At any rate their sum should not differ much from 90°; for the less the computed angle differs from 90°, the less chance there will be of any considerable error in the intersection.

Romney-Marsh, from its levelness, as well as other advantageous circumstances attending its situation, which the bare inspection of the map will render obvious, seeming to me to afford the best base of verification for the last triangle, I have given the series the shortest direction from Hounslow-Heath to that part of Kent. The right hand stations occupy in general the heights which extend across the Wealds. Those on the left are placed on the great range of chalk hills, which end, on our side of the Channel, between Folkestone and Walmer-Castle, and re-commence on the opposite side between Cape Blancnez and Calais.

It will be perceived, that I do not mean to make St. Paul's a station in the suite; because in that case Harrow and Hampstead must likewise have been made use of, all three extremely inconvenient for the reception of the great instrument. Besides, Greenwich Observatory being hidden from the country to the south-west by the Norwood heights, and from that to the south-east by Shooter's-Hill, after having made the detour of Harrow and Hampstead, and come across the smoke of the
Capital,

Capital, we should still have been obliged to make use of the two stations of Norwood and Shooter's-hill, without procuring so good an intersection of Bottle-hill, called in the common maps (I believe erroneously) Botley-hill, as is obtained by means of the station at the Hundred-acre-house. But although none of the stations of the series actually fall within London, nevertheless, from those in its vicinity, *viz.* the Pagoda, Norwood, Greenwich Observatory, and Shooter's-hill, we shall equally have it in our power to determine accurately the situations of Harrow, Hampstead, and St. Paul's, as well as many other chief steeples within the limits of the Capital.

Another principle I have endeavoured to adhere to, in the disposition of the triangles, is this, that, after having obtained sides in length from 12 to 18 miles, I continue them at that length as much as the circumstances will permit; for, if they came to be reduced considerably below that extent, the obvious advantages of a long base at the outset would be lost, by the subsequent contraction towards the close of the operation.

The tower of Tenterden church, being a very conspicuous object, may be seen every where from the summit of the chalk hills, as far west as the river Medway. It likewise may be seen from the eastern extremity of the second base, whereby the last triangle, *Tenterden, Lid, Allington Knoll*, is proposed to be verified. This knoll is itself a very remarkable object, more accessible, and in other respects more proper too for the purpose of a station than Lymne church steeple, which I had at one time thoughts of occupying. The high ground which separates Romney-marsh from the Wealds of Kent, passes immediately behind *Ruckinge*, that is to say, to the north-westward of it, and may therefore probably prevent the top of the chalk hills from being seen from the west end of the base

of verification; but if *Tatterlees-Barn**, or any other point on the range near it, can be seen from Ruckinge, then the station on the knoll, as well as that at Lymne, will become equally unnecessary, and the triangle of verification will become *Tenterden, Lid, Tatterlees*.

It will be perceived, that I propose to have a station on *Fairlight Head*, a land of considerable height, from whence there is a good view of the coast of France near Boulogne. From this point and Tatterlees, with the help of the Indian lights, I have no doubt of obtaining a fine intersection of the signal of the *Boulemborg*, a hill of some note behind the town of Boulogne, and one of the stations made use of by the French Academicians in the execution of their triangles. The advantages of obtaining a triangle of this magnitude, whose sides are respectively in length about 45, 36, and 25 miles, are too obvious to require any comment.

The high chalk cliffs near Folkestone prevent *Dover Castle* from being seen from Lid, or any where in the plain of Romney-marsh. Hence it will become necessary to form two small triangles to the northward of Tatterlees, in order to obtain an intersection of one of the turrets of the keep of that castle. Of the center of the keep, it will be perceived, by the strong dotted lines, that the French Academicians have procured, from their stations at *Calais, Blancnez, and Audingben*, an acute intersection (*de la Grosse Tour de Douvres*) making in the whole an angle of $28^{\circ} 16' 20''$.

The points which are obviously the best for connecting our triangles with those of our neighbours, are the *Boulemborg, Blancnez, and Calais*, provided we could by any means obtain

* Tatterlees Barn, in Packer's Map of East Kent, on the summit of the chalk hills, 727 feet above the sea.

as good an intersection of the last as we are certain of getting of the two first; but the breadth of the range of chalk hills being little different on our side from what it is on theirs, by confining ourselves to such a base as they will afford us, we cannot any way obtain an intersected angle at Calais greater than about 29° or 30° .

Thinking that possibly from St. Peter's church in the isle of Thanet the tower of *Notre Dame* at Calais may be seen, I have extended dotted triangles into that part of Kent; because, if the united heights should not be sufficient to raise the top of the tower above the curvature of the sea, which is the only thing to be doubted, we are always certain, that the signal of *Blancnez* may, by means of the Indian lights, be easily seen, since the whole range of chalk hills behind Calais are discovered with the naked eye from the isle of Thanet, when the weather is tolerably clear.

Having in this manner ascertained the relative situations, with regard to the coast of England, of three points on the coast of France, forming a triangle whose sides and angles we already know from their trigonometrical operations, we shall in like manner be enabled to determine the situation of the point M near Dunkirk, where the meridian of the Royal Observatory of Paris intersects a line drawn from the great tower of Dunkirk to that of *N. D.* at Calais. (See Tab. IX.) The distance MP, on the meridian of Paris, will then be had; and that being added to 133417 fathoms, the distance of M northward from the Royal Observatory, we shall have the total terrestrial arc, comprehended between the parallels of the two Observatories, answering to an arc in the heavens of $2^{\circ}38'26''$, or a difference of latitude between $51^{\circ}28'40''$ and $48^{\circ}50'14''$.

In like manner the distance from Greenwich to P, on the parallel of Greenwich, will then be readily computed, answering to the difference of longitude between the two Observatories; which, as far as can be judged from the map of Kent, corrected for the error in the direction of its meridian, amounts to about $2^{\circ} 20' 20''$, supposing always that no uncertainty remains with regard to the position of the point M. But here some remarks become necessary, which may probably suggest to the Academy of Sciences, that a further investigation of this matter may be needful on their part.

By referring to the 57th page of the first part of M. CASSINI's Book (*La Méridienne vérifiée*), it will be seen, that Dunkirk, by one series of triangles, is eastward from the meridian of Paris 1426.53, and by another 1414.29 toises, whereof the mean is 1420.41, equal to 1514 fathoms. This difference of $6\frac{1}{2}$ fathoms, or little more than half a second of longitude between the mean and extreme places of M, is certainly very inconsiderable. But in the 60th page, where, in verifying the meridian of Paris, by the comparison of the angle that *Broulezele* makes with the meridian of Dunkirk, and the angle of convergence of one meridian to the other, a difference of 21 seconds between $10^{\circ} 16' 13''$ and $10^{\circ} 16' 34''$, is alledged to be almost insensible, we do not think to be a conclusion so unexceptionable. This, however, is not the only cause of uncertainty with regard to the just position of the point M: one of more importance arises, from the difference that is found by two sets of triangles in the angle of intersection of the meridian of Paris, with a line drawn through M from the tower of Dunkirk to that of Calais.

Thus, by p. 53. and 56. of the first part of M. CASSINI's book, Dunkirk being the station, *Broulezele* makes an angle with

with the meridian of $10^{\circ} 18' 25''$ towards the south-west: and the angle between Broulezele and Hondscote being $78^{\circ} 11' 42''$, their difference $67^{\circ} 53' 17''$ is the angle that Hondscote is south-east from the meridian; wherefore the complement of this last angle to 180° , *viz.* $112^{\circ} 6' 43''$ is the angle that Hondscote makes with the meridian of Dunkirk produced northward. By p. 166. of the second part, Dunkirk being the station, the angle between Hondscote and Mont-Cassiel is shewn to be $51^{\circ} 7' 15''$; that between Mont-Cassiel and Watten $42^{\circ} 6' 35''$; and by p. 167. that between Watten and Calais is $51^{\circ} 40' 20''$. The sum of these three angles is $144^{\circ} 54' 10''$, from which deducting $67^{\circ} 53' 17''$ the angle that Hondscote is south-eastward from the meridian, there remain $77^{\circ} 0' 53''$ for the angle of Calais south-westward from it; and the complement of this angle to 180° , *viz.* $102^{\circ} 59' 7''$, becomes the angle that the meridian of Dunkirk produced northward makes with a line drawn through M to Calais: to which last adding the angle of convergence of one meridian to the other $1' 50''\frac{1}{2}$, corresponding to the distance of 1514 fathoms, equal to $1' 29''\frac{1}{2}$ of a great circle, we shall have $103^{\circ} 0' 57''\frac{1}{2}$ for the angle which the meridian of Paris produced northward from M makes with the line joining Dunkirk and Calais.

Again, by p. 63. of the third part of M. CASSINI's book, Dunkirk being the station, the angle that Gravelines makes with the meridian south-westward is $72^{\circ} 11' 48''$; and by p. 12. of the said third part, the angle between Watten and N. D. Calais is $51^{\circ} 39' 50''$: also that between Gravelines and Calais is $56^{\circ} 42' 0''$. Now the difference between these two last $4^{\circ} 47' 50''$ being added to $72^{\circ} 11' 48''$, we shall have $76^{\circ} 59' 38''$. and its complement $103^{\circ} 0' 22''$, for the angles that the meridian of Dunkirk makes with the line drawn from thence through the point M to Calais: to which last angle adding

the former convergence $1' 50''\frac{1}{2}$, we have $103^{\circ} 2' 12''\frac{1}{2}$ for the angle that the meridian of Paris produced northward from M makes with the said line; but by the former set of angles it was found to be only $103^{\circ} 0' 57''\frac{1}{2}$, wherefore the difference is $1' 15''$.

From M. CASSINI's book it appears, that Dunkirk is north from Paris 125515.25 toises, which make 133768 fathoms; and the point M being south from the tower of Dunkirk 351 fathoms, there remains for the distance of M northward from the Royal Observatory 133417 fathoms. Now, with this distance as radius, the value of an angle of $1' 15''$ is $48\frac{1}{2}$ fathoms, equal to $4'' 34''$ of longitude. Thus the point M, instead of being westward from Dunkirk 1514 fathoms, will, by the last set of angles, only be removed from it 1465 $\frac{1}{2}$ fathoms: wherefore the difference between the mean and extreme places of M, in this way of considering it, will amount to $24\frac{1}{2}$ fathoms, about four times as much as that resulting from the comparison stated in the 57th page. In the parallel of Greenwich the extreme difference will amount to 58.4 fathoms, or about $5\frac{1}{2}$ seconds of longitude, not much more than one-third part of a second of time.

In this sort of uncertainty, with regard to the precise point of intersection of the meridian of the Royal Observatory of Paris with the line joining Dunkirk and Calais, the only thing that can be done on our part, is to consider the mean position of M as just, that is to say, to suppose it to be 1514 fathoms westward from the great tower of Dunkirk, and having connected it with the British triangles, to shew then what angle its meridian will make with the line drawn from Dunkirk to Calais.

Comparison

Comparison of the celestial arc of the meridian, comprehended between the parallels of Greenwich and Perpignan, with the corresponding portions, measured and computed, of the terrestrial arc of the said meridian, between M. and Perpignan.

In the consideration of this matter it is to be observed, that M. CASSINI has divided the celestial arc between the parallel of Dunkirk, or, which is the same thing, between the parallel of M. and Perpignan, into four principal sections; viz. that from M. to Paris, from Paris to Bourges, from Bourges to Rodés, and from Rodés to Perpignan; assigning to each section the measured portion of the corresponding terrestrial arc, resulting from the triangles of the meridian.

By pages 110, 111, 112, of the first part of the book it appears, from the mean observed zenith distances of four stars, that the arc of the heavens, between the parallels of Dunkirk and Perpignan, contains

8 20 2 26

And from the mean of the observations of a like number of stars, the parallel of Rodés is distant from that of Dunkirk,

6 50 51 14

Wherefore Rodés is north from Perpignan

1 39 11 12

Bourges, by the mean zenith distances of two stars, is south from Dunkirk,

3 56 59 55

Wherefore Bourges is north from Perpignan,

4 23 2 31

And by another mean, in p. 112. it is

4 23 2 35

The mean of which two means gives for the distance of their parallels,

4 23 2 33

Perpignan

Perpignan is south from the Royal Observatory at Paris, by the	°	'	"	'''
mean zenith distances of four stars,	6	8	11	52
Hence Paris is south from Dunkirk	2	11	50	34
Rodés is south from Paris, by the mean zenith distances of two				
stars	4	28	59	45
Hence Perpignan is south from Rodés,	1	39	12	7
And by a former result it was found to be,	1	39	11	12
Hence the mean of the two means gives, for the distance of the				
parallels of Rodés and Perpignan,	1	39	11	28

The tower of the great church at Dunkirk is northward from what was the station of the sector $84\frac{1}{4}$ toises, equal to 89.8 fathoms, which correspond to an arc in the heavens of

Wherefore the tower is north from Paris,

But the point M is south from the tower 351 fathoms, which answer to an arc in the heavens of

Hence the point M is northward from the Royal Observatory at Paris,

Now, from these *data*, together with the latitude of the Royal Observatory at Greenwich $51^{\circ} 28' 40''$, and that of Paris $48^{\circ} 50' 14''$, we shall have the latitudes of the several stations between Greenwich and Perpignan, with their differences, or the celestial arcs comprehended between them, as underneath.

Stations.	Latitudes.	Diff. or celestial arcs.
Greenwich Royal Observatory,	$51^{\circ} 28' 40''$	$0^{\circ} 26' 50'' 52'''$
Point M near Dunkirk,	$51^{\circ} 1' 49'' 8'''$	
Paris Royal Observatory,	$48^{\circ} 50' 14''$	$2^{\circ} 11' 35'' 8'''$
Bourges,	$47^{\circ} 5' 44''$	$1^{\circ} 45' 9'' 19'''$
Rodés	$44^{\circ} 21' 13'' 36'''$	$2^{\circ} 43' 51'' 5'''$
Perpignan (<i>St. Jaumes</i>)	$42^{\circ} 42' 2'' 8'''$	$1^{\circ} 39' 11'' 28'''$

This latitude of Perpignan $42^{\circ} 42' 2'' 8'''$ is what results from the immediate comparison of the lengths of the celestial arcs,

arcs, as determined by the zenith distances of stars, taken with a sector of six feet radius, and where the observations are so nearly consistent among themselves, as to leave little doubt of their accuracy; but in the 290th page of M. CASSINI's book, so often quoted, as well as in the 170th page of his *Description Géographique de la France*, published in 1783, the latitude of Perpignan is given $42^{\circ} 41' 55''$, which is $7'' 8'''$ less than that deduced from the observations, without any reason that I can perceive being assigned for the reduction.

Perpignan, the southernmost station of the meridian line extending from Dunkirk through the whole kingdom of France, is situated at no great distance from the bottom of the Pyrenean mountains, where that lofty range ends at the Mediterranean sea. M. DE LA CAILLE was of opinion, that the plummet of the sector must have been affected by the attraction which it would suffer from that cause; a supposition which, nevertheless, has been doubted, since the observations made in this country on the attraction of *Schehallion*: for by these it appeared, that the effect, although sensible, was but small, even when the sector was placed as near as possible to the opposite sides of the mountain. It is indeed true, that the *Canigou*, the highest of the Pyrenean range, being situated obliquely to the meridian, and at a considerable distance from Perpignan, would not probably occasion much deviation in the plummet; yet, on the other hand, when we compare the very trifling quantity of matter in *Schehallion* with the immensity of the mass in the Pyrenees, in the direction of the meridian, I cannot help being of M. DE LA CAILLE's opinion, that the plummet of the sector would be sensibly affected, that is to say, it would be drawn to the southward out of its perpendicular direction, and would thereby give the zenith distance of the pole,

pole, or any other northern star, too little, and consequently a latitude too great. Until triangles shall have been extended beyond the Pyrenees, and the sector placed on the south side of the range, the quantity of this attraction (by its double or counter-effect) cannot possibly be ascertained. I will, however, only suppose it to have been $10'' 8'''$ to be deducted from the latitude of Perpignan, which will then become $42^{\circ} 41' 52''$, only three seconds less than that assigned to it in M. CASSINI's two books before mentioned. Thus the arc between Rodés and Perpignan will be $1^{\circ} 39' 21'' 36'''$, and the total celestial arc between Greenwich and Perpignan will be $8^{\circ} 46' 48''$, as may be seen by attending to the four columns towards the left-hand of the annexed table of comparison.

With regard to the corresponding terrestrial arc, under which head are arranged the eleven columns towards the right-hand of the table, it is to be observed, that various measurements have at different times been made in different latitudes of the lengths of the degrees of the meridian, for the purpose of obtaining, within certain limits at least, the true figure and dimensions of the earth. The most essential operations of this sort, as having been executed with most care, with the best instruments, and at the greatest distances from each other, have all been done within these last forty or fifty years; namely, in Peru under the equator, in middle latitudes in France and Italy, and in Lapland near the polar circle. The attraction of mountains, and unavoidable errors in the execution, will ever prevent just conclusions from being drawn from the comparison of measurements made too near each other. These last will always be found to differ more or less among themselves. Sometimes even the results may become absurd or contradictory. In cases of this sort, a mean of several should no doubt be taken

taken for a mean latitude, unless there should be sufficient grounds for rejecting any from the number, as differing too much from the others. Hence it is, that philosophers are not yet agreed in opinion with regard to the figure of the earth; some contending, that it has no regular figure, that is to say, not such as would be generated by the revolution of a curve around its axis. Others have supposed it to be an ellipsoid; regular, if both polar sides should have the same degree of flatness; but irregular, if one should be flatter than the other. And, lastly, some suppose it to be a spheroid differing from the ellipsoid, but such as would be formed, nevertheless, by the revolution of a curve around its axis; although in this case too one polar side may not be similar to, but more or less oblate than the other.

In order, therefore, to put this matter in its true light, and to enable every one to judge, by simple inspection only, which of the theories agrees best with actual measurement, I have computed on ten different *hypotheses*, and arranged in their order, the lengths of the arc between Greenwich and Perpignan; as also some other chief properties of each figure, which last fill up the space towards the bottom of the table. This mode of collecting the results seemed to me to be the most distinct that could be followed, to avoid that perplexity which must for ever occur in referring back to, and comparing, many numbers together, computed on different systems, when the whole are not placed before the eye at once.

The first of the eleven columns, or that which comes next to the celestial arc, contains the measured portions of the corresponding terrestrial arc, as far as they have yet been executed. The blanks at top cannot be supplied, until we shall

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have determined the length MP, in the map, being the space comprehended between the parallels of M and Greenwich.

In the second column are arranged the computed dimensions appertaining to the earth as a sphere, supposing its semi-diameter to be a mean between the longest and shortest of M. BOUGUER's second spheroid. It is from the magnitude of this sphere that I compute the degrees of a great circle for the sides of spherical triangles. By adverting to the errors or differences between the measurement and computation, in their respective places, it will obviously appear that the earth differs very considerably from a sphere: for although the arc M Perpignan of $8^{\circ}\frac{1}{4}$ only exceeds the truth by 609 fathoms; yet an arc of equal length at the equator, viz. $8^{\circ}.33 \times 374.6$, would give an excess of 3120 fathoms; and at the polar circle $8^{\circ}33 \times 335.2$ would give a defect of 2792 fathoms.

After the sphere follow seven ellipsoids of different degrees of oblateness, from the first, whose semi-diameters have to each other the ratio of 179.047 to 178.047, to the seventh, where it is only that of 540 to 539. On the principles which have served as the foundation of the first and second, it will be necessary to make some remarks; but as to the others, a few words will suffice for each.

With regard to the first ellipsoid, supposing the earth to be homogeneous, it is well known, that the ratio of its semi-diameters may be found, by comparing with each other the lengths of the pendulums that vibrate seconds in different latitudes; which lengths are deduced from the seconds of acceleration, that the pendulum, so adjusted, and unalterably fixed as to length, at the equator, would perform in 24 hours, on being successively transported to different latitudes, as far as the pole, where the force of gravity being the greatest, the acceleration would likewise be the greatest. The calculations

lations for this purpose were first made soon after * Lord MULGRAVE's return from his Voyage towards the North Pole in 1773.

* The most northern experiments hitherto made with the pendulum, are those at Spitzbergen, in latitude $79^{\circ} 50'$, whereof an account has been given in the Voyage towards the North Pole in 1773. The machine made use of on that occasion belonged originally to the celebrated watch-maker, Mr. GRAHAM, and was lent for the purpose by its present proprietor, Mr. CUMMING, who has since obligingly permitted it to remain for several years, at two different times, in my possession, where it now is. Soon after the return from Spitzbergen, I ascertained its rate of going by my observatory clock, for a great while together, in very different temperatures; and thereby found, that the variation from heat and cold, namely, $\frac{5}{8}$ ths of a second for each degree of FAHRENHEIT, was considerably more than had been allowed for it, in determining the acceleration from London to Spitzbergen. About this time likewise, Dr HORSLEY discovered that an error had been committed by the astronomer (the late Mr. ISRAEL LYONS) employed by the Board of Longitude on that Voyage, in calculating the spherical triangle for the correction of the time between the 16th and 17th of July, on account of the obliquity of the transit-instrument. From Dr. HORSLEY's printed letter on this subject in 1774, it appears, that the difference from that cause amounted to 37 seconds in time, as given by the observation with the instrument and by the watch. And since there was reason to suspect, that the telescope had by accident been moved (one could not tell how much), it was judged safest to adhere to the acceleration $71''.08$ as given by the watch. The correction for the greater contraction of the steel rod, I found to be $2''.58$ to be subtracted, wherefore the acceleration from London to Spitzbergen became $68''.5$, and that from the equator to London being $156''$, the total acceleration from the equator to Spitzbergen consequently was $224''.5$. Now, supposing the length of the pendulum at the equator to be, as M. BOUGUER made it, just 38.9949 inches, we shall have its length at Spitzbergen 39.1978 inches; and thence the ratio of the semi-diameters of the earth, considered as an homogeneous ellipsoid, will be that of 193.1 to 192.1, instead of 183.7 to 182.7, which the acceleration $72''.7$ uncorrected would have given. And here it seems necessary to mention some other mistakes of computation, inadvertently fallen into by the astronomer, in deducing the ratios of the semi-diameters of the earth, as stated in the 179th page of the Voyage to the North Pole, not hitherto noticed that I know of.

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1773. I am aware that experiments with the pendulum have not yet been made with that accuracy that the delicacy of their nature

With the accelerations, indicated in that page as arising from different experiments, or depending on the systems of different philosophers, the length of the Spitzbergen pendulum, and consequently the ratios, should have stood as in the annexed table, with which it will be perceived they differ very considerably. Sir ISAAC NEWTON's ratio is placed last, as probably differing most from the truth.

Experiments.	Acceleration			Length of the Spitzberg. pendulum.	Ratio of the femi diameters of the earth.
	From the equator to London.	From London to Spitzberg.	Total		
Said to be, but erroneously, Sir I. NEWTON's	156"	+ 66.9	222.9	In. 39.1964	194.5 to 193.5
Mr. CAMPBELL's		+ 76.6	232.6	39.2052	186.5 to 185.5
M. MAUPERTUIS's		+ 86.5	242.5	39.2141	178.8 to 177.8
Ld MULGRAVE's correct.		+ 68.5	224.5	39.1978	193.1 to 192.1
Sir ISAAC NEWTON's		+ 32.4	188.4	39.1652	230. to 229.

In this manner the lengths of the pendulum having been found for all the latitudes where the best or most consistent experiments had been made on its acceleration, and these lengths having been successively compared with each other from Spitzbergen to the equator, 119 results in the whole were produced. But as this number comprehended the comparisons of those at the Cape of Good Hope and the Isle of France, both in south latitude, these being thrown out, as well as the Porto Bello pendulum, and some few others more irregular than the rest, there remained at last 75 results, the arithmetical mean of which gave the ratio 179.047 to 178.047, as mentioned in the text.

In case Mr. CUMMING's machine should at any time hereafter be employed in the same sort of experiments, it may be proper to observe, that the diameter of the brass ball is 3.906 inches, and its weight 63726 Troy grains. The weight of its bulk of mercury is 106980 Troy grains. Hence the weight of mercury is to the weight of this ball of brass, in air of the heat of 62° of FAHRENHEIT, as 1.678752 to 1; and the weight of this brass to air is as 7673 to 1. The experiments for this purpose, in which Dr. GEORGE FORDYCE assisted, were made in the house of Mr. ALCHORNE, at his Majesty's Mint in the Tower, on the

nature seems to require, and which at some future period should, and probably will, be undertaken on a more extended plan, with the very best machines that can be constructed for the purpose. Many ages may elapse before measurements of any considerable portion of the surface of the earth can be made in very high southern latitudes, so as to determine, with any tolerable degree of exactness, whether the southern spheroid be similar to the northern, supposing both to be figures of revolution. But by good experiments with the pendulum alone, easily repeated in all latitudes, the ratio of the semi-diameter of the equator to the semi-axis on both sides may be readily obtained. In the mean time I thought it might be useful to shew the result of a comparison of the most consistent experiments of that sort, that have hitherto been made in different latitudes, after having applied to those at Spitzbergen certain corrections, which seemed necessary, as further explained in the note. Thus it appears, that the arithmetical mean of 75 comparisons between Spitzbergen and the equator gives the

the 8th of April and 6th of June, 1776; and the beam made use of, with 15 lbs. weight in each scale, was true to three grains.

The machine, in its present state, although better fitted for experiments than it was originally, since it has a wheel added, whereby it registers its own time, and now goes for 12 hours without winding up, is no longer the same that performed at Spitzbergen: for in the interim of the two times it has been in my possession, the steel rod having by accident been broken, Mr. CUMMING substituted another, as like to the former as possible with regard to size, but which will, in all probability, be susceptible of a different expansion and contraction from the effects of heat and cold. In the application of the machine, I have usually loaded the top of it with about 24 lbs. weight of lead, in order to render the center of suspension as little liable to motion as possible: yet, notwithstanding this precaution, a pointed plummet, suspended to the top of the frame, has a small degree of counter-vibration to that of the ball, which no doubt must produce some effect.

ratio

ratio of the semi-diameters formerly mentioned 179.047 to 178.047. On this hypothesis the arc MP should contain 27350 fathoms. The error on the total arc M Perpignan amounts to 2078 fathoms. M. BOUGUER's degree at the equator being adhered to, the 45th of latitude will exceed the truth 216, and that at the equator 148 fathoms,

The ratio of the semi-diameters of the second ellipsoid, which comes now to be spoken of, has been obtained by the comparison of such measured lengths of the degrees of the meridian in different latitudes, as have been found to be most consistent with each other. Our countryman, Mr. NORWOOD, was the first, of late times, who made any attempt of this sort. But the measurement, executed by him in the year 1633, between London and York, has no pretence to exactness, since he himself tells us, *that when he did not measure, he paced!* Besides, his degree is as great, or even greater, than that in Lapland; and these are surely sufficient reasons for rejecting it from the comparison. The degree measured by M. LIESGANIG in latitude $45^{\circ} 57'$, in that part of Poland lately fallen to the share of the Emperor and annexed to Hungary, being so much shorter than degrees to the southward of it, gives grounds to suspect, that some error had crept into that operation, or that the plummet had been affected by the attraction of neighbouring mountains, and therefore is not made use of on the present occasion. M. DE LA CAILLE's degree at the Cape of Good Hope, being in south latitude, and so much greater than those of the same height in northern latitudes, is improper likewise to be brought into the comparison, lest the difference may have arisen from a dissimilarity in the two polar sides of the ellipsoid. The degree measured in the north of France, compared with that in Austria, coming out
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absurd, it has been judged best to take a mean between them for a mean latitude. In like manner the latitudes of the two Italian degrees differing but little from each other, a mean length has been taken between them for a mean latitude. Accordingly the latitudes and the measured lengths of the degrees which, in the second ellipsoid, have been compared together, will appear as underneath :

Observers names.	Countries.	Latitudes.	Measured lengths.
BOUGUER,	Peru, -	0 0	- 60484.5
MASON and DIXON,	Maryland, -	39 12	- 60628.5
BOSCOWICH,	Italy, -	43° 0' } 43 52	{ 60725.5 } 60773.4
BECCARIA,	Piedmont -	44 44 }	{ 60821.3 }
CASSINI, &c.	Middle of France,	- 45 0	- 60777.6
LIESGANIG,	Austria, -	48 43 }	{ 60839.4 } 60833.0
CASSINI, &c.	North of France, -	49 23 }	{ 60826.6 }
MAUPERTUIS, &c.	Lapland, -	66 20	- 61194.3

Now these six degrees, being successively compared with each other, fifteen results are thereby obtained, whereof the arithmetical mean gives for the ratio of the semi-diameters of the ellipsoid that of 192.483 to 191.483. By adverting to the table, it will further appear, that the arc MP should be in length 27331 fathoms. The arc M Perpignan exceeds the truth 1758, the 45th of latitude 180, and that at the polar circle near 88 fathoms.

The ratio of the semi-diameters of the third ellipsoid 216.06 to 215.06 is obtained by adhering to the measured lengths of the degrees at the equator and polar circle. According to this hypothesis the arc MP should contain 27301 fathoms. The arc M Perpignan exceeds the truth 1288, and that at the 45th of latitude more than 128 fathoms.

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The ratio of the semi-diameters of the fourth ellipsoid 222.55 to 221.55 is the same, as may be seen by referring to the table, with that assigned by M. BOUGUER to his first spheroid, where the increments to the degrees of the meridian above that at the equator are as the second power or squares of the sines of the latitudes. It was intended chiefly to shew how small the difference is between the magnitudes and nature of the curves of the two figures. The arc MP should contain 27294 fathoms. The arc M Perpignan errs in excess 1177 fathoms. The 45th degree exceeds the truth 116 fathoms; and that at the polar circle falls short of the measured length 21 fathoms: M. BOUGUER's degree at the equator being adhered to as the standard.

The ratio of the semi-diameters of the fifth ellipsoid, 230 to 229, is that assigned to the earth by Sir ISAAC NEWTON. On this hypothesis the arc MP should contain 27241 fathoms. The arc M Perpignan only exceeds the truth 202 fathoms, because the 45th degree of the meridian is here adhered to as the standard length. But then the degree at the equator falls short of the measurement 102 fathoms, and that at the polar circle 146½; wherefore, an arc of $8^{\circ}\frac{1}{2}$, in the first case, would be defective 850, and in the last 1220 fathoms.

The ratio of the semi-diameters of the sixth ellipsoid, 310.3 to 309.3, is obtained by adhering to the measured lengths of the degrees at the equator and 45th of latitude. The arc MP should contain 27250 fathoms. The arc M Perpignan only exceeds the truth 131 fathoms; but on this hypothesis, the degree at the polar circle would be defective near 217 fathoms, and consequently on $8^{\circ}\frac{1}{2}$ the error would be 1807 fathoms.

The seventh or last ellipsoid, being that of the least flattening, has for the ratio of its semi-diameters 540 to 539. The arc MP should contain 27206 fathoms. The 45th degree of latitude being adhered to as the standard, the arc M Perpignan would only exceed the truth by 46 fathoms; but, on the other hand, the degree at the equator erring in excess 124½ fathoms, and that at the polar circle being defective near 303; therefore, in the first case, the error on 8°½ would be 1037, and in the last 2524 fathoms. Hence it is obvious, that the arcs of an ellipsoid, however great or small the degree of its oblateness may be, will not any way correspond with the measured portions of the surface of the earth: for if we retain the length of M. BOUGUER's degree at the equator as the standard, and make the ellipsoid extremely flat, as in N° 1. the figure will become too prominent in middle latitudes, that is to say, the curve will rise above the real surface of the earth, and, in proportion to the excess of the radius, will always give degrees that exceed the measured length. On the contrary, if we give the ellipsoid a small degree of flatness, as in N° 7. and adopt the measured length of the 45th degree as the standard, the measured and computed arcs will nearly agree in middle latitudes; but at the equator the curve will rise very considerably above the surface, and will thereby give degrees that are too great; while at the polar circle it will fall below it, and give degrees that are too little in the proportion of about 2½ to 1 compared with the error at the equator. From all which we may conclude, that the earth is not an ellipsoid.

The two columns towards the right-hand of the table, contain the arcs of two spheroids differing from the ellipsoid. The first is that adopted by M. BOUGUER as his first hypothesis, where the increments to the degrees of the meridian

above that at the equator follow the ratio of the second power or squares of the sines of the latitudes, and to which he has suited his first table of degrees, N° 32. p. 298. This spheroid differs but insensibly, as has been already mentioned, from the fourth ellipsoid. They have both the same semi-diameters; but the arcs of the spheroid being somewhat longer than those of the ellipsoid, the former thereby becomes, in a trifling degree, more prominent in middle latitudes. On this hypothesis the arc MP should be in length 27295 fathoms; M Perpignan exceeds the measurement 1196 fathoms; and the degree at the equator being adhered to as the standard, the 45th errs in excess 118, while that at the polar circle is defective only 20 fathoms.

The second spheroid is that whereon M. BOUGUER founded his second hypothesis, which supposes the increments to the degrees of the meridian, above that at the equator, to follow the ratio of the fourth power or squared squares of the sines of the latitudes, and to which he has adapted his second table of degrees N° 38. p. 305. It will be perceived, that the ratio of the semi-diameters of this spheroid, *viz.* 179.4 to 178.4 differs little from that appertaining to the first ellipsoid; but here the curve falling considerably within, that is to say, being less prominent than the ellipsoid in middle latitudes, the arcs are thereby contracted in such a manner as to agree within 5 fathoms with the measured length of the meridian of France, in an extent of about $8^{\circ}\frac{1}{2}$, comprehended between M near Dunkirk, and Perpignan situated at the bottom of the Pyrenean mountains. By inspection of the table it will further appear, that the errors in the several sections of this arc are not only small, but they are sometimes *plus* and sometimes *minus*, a never failing proof that, as far as our present *data* will enable

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us to judge, the figure here assigned to the earth, notwithstanding what has been alledged to the * contrary, is exceedingly near the truth. According to this hypothesis, the distance MP on the meridian of Paris, which is yet to be determined by our trigonometrical operations, should contain 27243 fathoms, being only 35 fathoms less than what is given by the mean of the seven different ellipsoids, a space not amounting quite to 2'' of latitude. The result of the measurement of this space, answering to an arc in the heavens of 26' 50'' 52''' of latitude, will be a further confirmation, or otherwise, of the justness of the theory. The degree at the equator being adhered to as the standard, it will be seen from the table, that the 45th is defective 37.6, while that at the polar circle errs in excess 9.4 fathoms.

* Mr. J. KLOSTERMANN, Inspector of the Corps of Pages at St. Petersburg, in his manuscript Memoir, some time since transmitted to the Royal Society, to the Academy of Sciences at Paris, and also to that at Gottingen, has endeavoured to shew, that the French trigonometrical operations are extremely erroneous. It would seem, nevertheless, that he has attempted to prove too much. He should certainly have confined his criticism to the triangles of the meridian only, which are distinguished from the others in M. CASSINI's book, by being printed in larger characters, without drawing conclusions from very acute angles, which, although inserted in the general register, were not made use of in the determination in question. But as the Royal Academy of Sciences will, no doubt, vindicate the credit of their own operations, I shall only further remark, on the Literary News from Gottingen (*Nachricht aus den Göttingischen Anzeigen von gelehrten Sachen*, 117 Stück, 1785), which accompanied the said Memoir, and where it is said, "That M. BOUGUER's hypothesis of the 4th power fell to the "ground, so soon as other degrees were measured than those on which he had "founded it," that I confess myself to be quite of a different opinion, not doubting, that when the comparison is fairly drawn between this and every other system that has hitherto been submitted to the consideration of the public, M. BOUGUER's will be found to be justly entitled to the preference, which I have here endeavoured to give it. His works shew, that he was a man of very superior abilities, eminent as a mathematician, and perhaps the best practical one that ever existed.

Besides these two spheroids of M. BOUGUER, I had computed the arcs of another between the two, where the increments to the degrees of the meridian were in the ratio of the fractional power $3\frac{4}{100}$ of the sines of the latitudes. Thus the computed agreed accurately with the measured dimensions in three principal parts of the earth's circumference, namely, at the equator, 45° and $66^\circ 20'$ of latitude, at the same time that the arc MP contained 27257 fathoms; but the labour of computation being hereby greatly augmented, and the error on the total arc between M and Perpignan amounting to 290 fathoms in excess, this system did not seem to me to deserve to be put in competition with the simplicity of that of M. BOUGUER, who indeed, for the same reason, preferred the fourth to the fractional power $3\frac{1}{11}$ ths, which he tells us was that which still came nearer the truth. In short, it must be from the results of future operations executed in very high latitudes, and the measurement of degrees of longitude on the equator, that we can hope to have sufficient authority for any correction or amelioration of the system of M. BOUGUER.

Differences of longitude.

Hitherto there has been no particular reference to some lines at the bottom of the table, containing the computed lengths of degrees of longitude on each hypothesis, in three different latitudes, namely, the equator, $43^\circ 32''$ and $51^\circ 28' 40''$. No measurements of degrees of longitude, as far as I know, have ever been executed with sufficient care and accuracy, except that in the south of France, as mentioned in the 105th and 106th pages of M. CASSINI's book, which was determined by
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the repeated explosions of gunpowder in the open air, and found to contain 41618 toises, equal to 44354.4 fathoms. By attending to the table it will be seen, that the error in excess of M. BOUGUER's theory, on the length of this degree trigonometrically measured, amounts only to 19 fathoms, which is little more than $\frac{1}{10}$ th part of a second of time.

In fixed Observatories, where able astronomers have been for many years employed in repeating their observations of the heavenly bodies, it seems surprising, that any doubt should remain with regard to what is called the astronomical difference of longitude, or, in other words, the difference of time between them; yet it has been alledged, that an uncertainty of this sort exists, even with regard to the situation of Greenwich and Paris, which, reckoned by its extremes, extends to about 10 or 11 seconds, answering in the latitude of Greenwich to the enormous difference in space of between 1600 and 1700 fathoms! But it will be considered as still more wonderful, if between two British Observatories, Greenwich and Oxford, which have been long supplied with great and costly instruments of the very best kinds, there should remain an uncertainty in this respect of 2 or 3 seconds of time: for in the latitude of Greenwich 3 seconds correspond to 477, and in that of Oxford to 474 $\frac{1}{2}$ fathoms. These, however, are points which must be left to the respective astronomers to settle in the best way they can; and it is not to be doubted, that the Astronomer Royal will throw a new and very satisfactory light on the matter, in the Paper which he proposes about this time to lay before the Royal Society, along with M. CASSINI's Memoir, which, for that purpose, has now been nearly two years in his possession.

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With regard to the trigonometrical operation (which may be considered as infallible, because, by means of the base of verification, it will prove itself, and if small errors unavoidably arise in the course of a long suite of triangles the maximum of these may be always ascertained), I have no doubt that the distance between Greenwich and the point P in the map may thereby be determined to a very small number of fathoms, perhaps to fifteen or sixteen on a difference of longitude of about $2^{\circ} 20' 20''$, and therefore to about $\frac{1}{10}$ th part of a second of time on each degree. This, for any useful purpose, will certainly be admitted to be sufficiently near the truth, and is probably considerably nearer than it will be brought for many years to come, by a mean of the best observations of the heavenly bodies, if these should be found in the present state of the matter to leave it yet doubtful to two or three seconds.

The astronomical difference of time may likewise be obtained by experiments on the instantaneous explosion of light; but these I would propose to be made subsequently to the trigonometrical operations. The station of *Tatterlees*, towards the eastern extremity of our range of chalk hills, or some point near it, would seem to be the most proper for the place of explosion, because it can be seen from *Bottle-hill*, on the same range, and nearly in the meridian of Greenwich Observatory. It is not to be doubted, that *Tatterlees* may be seen from *Fienne Windmill*, or even perhaps from that of the *Brunenberg*; since they are both situations, on the continuation of the same range in France, the distance being shorter too, and little land, but chiefly sea, intervening. Let us then suppose, that the two astronomers with their clocks and transit-instruments are posted, one at *Bottle-hill*, and the other at the *Brunenberg*, while gunpowder is repeatedly exploded at *Tatterlees*, or while the Indian lights

lights are alternately exhibited, and again covered by an extinguisher prepared for the purpose, which operation may be repeated several times the same evening; it is certain, that a just mean being taken between the instants so marked by the respective clocks, well regulated before-hand, the difference of time between the two extreme stations will thereby be obtained to a very considerable degree of accuracy, and probably more to be relied upon than that resulting from the comparison of the observations of the heavenly bodies.

Rockets have been proposed by some to be applied in this way; but even the largest sort, carrying up too small a body of white light, could, I think, only be depended upon for short distances; and since the actual error of observation would be the same on a short as on a long distance, remote stations only should be chosen for conclusive experiments of this nature. It is imagined, that by means of a balloon, a set of light-balls might be sent up of such a volume as to be seen, from their great elevation in the air, at stations very remote from the place of explosion. The balloon would ascend with a burning fuze attached to the first ball, and having attained the wished-for height, it might be made fast below, by means of a cord fixed to it for the purpose*. The first ball, on its explosion, would communicate fire to the second, and so in succession, at intervals of time proportionable to the lengths of

* It is to be understood, that the practicability of fastening the balloon to the earth by means of a rope must be previously ascertained: for if that should be found impossible, without the wind forcing it down again to the ground, then it must be sent up detached with burning fuzes fixed to it, of still greater dimensions. By means of these it is imagined, that the track of the balloon in the air might be pointed out to the remote observers, who might perhaps be better enabled thereby to watch for and distinguish the successive instants of the bursting of the white lights.

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the fuzes. Thus 8 or 10 instants might be marked from the experiment of a single balloon, which might then be hauled down to be reloaded for a repetition. But whatever might be the mode adopted as the best for conducting experiments of this nature, the observers must not only be very attentive and diligent, but also quick-sighted, have their clocks nicely regulated indeed, and the trials must be many times repeated before the uncertainty, even in this way, which seems to be the best mode, could be reduced to less than $\frac{1}{100}$ th part of a second of time, to which it may infallibly be brought by trigonometry.

Having in this manner shewn what probable degree of exactness may be expected in the various, but usual, ways of ascertaining the difference of longitude between the Observatories of Greenwich and Paris, and compared the results with the uncertainty that seems yet to exist in this matter from the state of astronomical observations; let us next see how Mr. RAMSDEN's instrument is likely to perform, when actually applied to the determination in question, by the observed angle between the pole star in its eastern or western azimuth, and a very remote station, whose distance from the instrument is known by the series of triangles, and distinguishable by the Indian lights at night, for the purpose of this particular observation.

With an instrument, carrying telescopes so good that the pole star may be seen in daylight, it is obvious, that the bisected angle between the star in its eastern and western azimuths will give at once the polar distance of the star, and the true meridian of the place, as referred to any known stations visible at the time of observation. But as cloudy weather may often prevent a complete observation of this sort from being obtained, and since much time might be lost in

attempting it, therefore the declination of the star settled for any particular period being accurately known *, its apparent distance from the pole may, by the established rules, be readily computed for any proposed day, as well as the precise times of its greatest elongations, twice in 24 hours, when in its eastern and western azimuths, at which times it will, for several minutes, appear, as to sense, stationary or without motion, except in altitude. These are, therefore, the best times for taking the angle between the star and any particular station, since the observations may be repeated frequently in the space of a few minutes, or until it shall be perceived that the star has again approached towards the pole. Now, suppose the station of the instrument to be at *Tatterlees*, whose distance from the perpendicular to the meridian of Greenwich, and consequently from its parallel, is known by the trigonometrical operation. The latitude of the station becomes known likewise; and let the co-latitude be $38^{\circ} 54' 20''$. Let us likewise suppose the distance of *Bottle-hill* on one side to be 44100 fathoms, equal to $43' 28''.6$ of a great circle; and that of the *Brunenberg* on the other to be 38250 fathoms, equal to $37' 42''.6$ of a great circle; and further, that on these two stations the Indian lights are exhibited for the time proposed. Now, let the angle between the meridian and *Bottle-hill*, and that between it and the *Brunenberg*, be observed by means of the pole star corrected for its distance for the day; and suppose the first to be $75^{\circ} 10'$,

* Since this Paper was written, the Astronomer Royal has been so obliging as to furnish me with the mean distance of the pole star from the pole, as settled at Greenwich by eight observations above and nine below it, made in the year 1786; whereby it appears, that the mean distance, reduced to the beginning of that year, was $1^{\circ} 50' 8''.35$; and the mean annual precession in declination being $19''.55$, consequently, the mean distance for the 1st of January, 1787, was $1^{\circ} 49' 8''.8$.

and the last $125^{\circ} 5'$; thus we shall have two spherical triangles to compute, in each of which two sides, and the contained angle, are known, and one side, *vis.* the co-latitude, is common to both. Now, from these *data*, making use of the half sum and half difference of the sides,* we shall have the angles in these two triangles as * underneath, and the angle of longitude between *Bottle-hill* and the *Brunemberg*, equal to that at the pole, will be found to be $1^{\circ} 55' 56''.1$. If from this angle we deduct about $30''$ or $35''$, for the space that the *Bottle-hill* seems to be to the westward of the meridian of Greenwich, there will then remain $1^{\circ} 55' 21''$ for the east longitude of the *Brunemberg*, being very nearly that expressed in the map of the triangles which accompanies this paper.

* Bottle-hill.			Brunemberg.		
Half difference	-	$51^{\circ} 25' 14.05$	Half difference	-	$26^{\circ} 44' 12.2$
Half sum	-	$52^{\circ} 32' 25.07$	Half sum	-	$27^{\circ} 32' 57.3$
Angle at Bottle-hill		$103^{\circ} 57' 39.12$	Angle at Brunemberg		$54^{\circ} 17' 9.5$
Angle at the pole	-	$1^{\circ} 7' 11.02$	Angle at the pole	-	$48^{\circ} 45.1$
Contained angle	-	$75^{\circ} 10' 0.$	Contained angle	-	$125^{\circ} 5' 0.$
Sum of the three angles		$180^{\circ} 14' 50.14$	Sum of the three angles		$180^{\circ} 10' 54.6$
Angle of convergence		$52^{\circ} 20.88$	Angle of convergence	-	$37^{\circ} 50.3$
Excess above 180°	-	$14^{\circ} 50.14$	Excess above 180°	-	$10^{\circ} 54.6$
Angle of longitude	-	$1^{\circ} 7' 11.02$	Angle of longitude	-	$48^{\circ} 45.1$
Sum of the two longitudes $1^{\circ} 55' 56''.12$.					

It is to be observed, that the meridians, which are all parallel to each other at the equator, on their departure from thence converge more and more as they approach towards the pole, where the angle of convergence becomes equal to the angle of longitude. It may also be remarked, that the angle of convergence, augmented by the excess of the three angles of the spherical triangle above 180° , is always equal to the angle of longitude, or that at the pole. And as this holds universally in all latitudes, it affords a ready means of proving that the computations are just.

As far as we are enabled to judge at present, from the examination of the divisions of Mr. RAMSDEN's instrument, there is every reason to believe, that in taking angles around the horizon, the mean of several repetitions of the same angle, as referred to different parts of the circumference of the circle, will differ very little from the truth, so little indeed, that in many cases the error will totally vanish. But in elevating the telescope towards the pole, let us suppose that an error of 5 seconds on each of the contained angles at *Tatterlees* has been committed; and further, that even an error of 5 seconds of latitude, equal to about $84\frac{1}{2}$ fathoms on the meridian, may have been fallen into, in estimating the co-latitude (which never can happen, but is only here admitted, to place the example in the most disadvantageous circumstances possible); then whoever will give themselves the trouble to recompute the two triangles with these new *data*, will find the result in longitude not to be varied thereby, in the first case above $\frac{1}{3}$ th part of a second, or $\frac{1}{7\frac{1}{2}}$ th part of a second in time; and in the last not quite 1 second, or $\frac{1}{7\frac{1}{2}}$ th part of a second in time. Hence I conclude, that the best mode of determining the differences of longitude will be by the instrument itself, applied in this way, in taking the angles between the pole star and very remote stations, distinguishable at night by the help of the Indian lights, and whose distance is accurately known. This method will, it is true, be liable, as well as astronomical observations, to the imperfections of the instrument, particularly those of the telescope, and the unavoidable error in its application; but, on the other hand, it will be entirely free from the irregularities of clocks, and the imperfections of vision in marking the instantaneous explosion of light. When both methods have been repeated a sufficient number of times, with all

imaginable care, we shall then, and not till then, be able to judge to which the preference may be due. Thus five or six long stations, in or nearly in the parallel of Greenwich, such, for instance, as that of Shooter's-hill Tower, would reach from the east quite to the west of the island: and as a very considerable degree of consistency might be expected among the results for equal portions of the parallel, this method seems to be as likely as any to furnish *data* for determining the nature of the spheroid or figure of the earth.

Table of the degrees of the earth, constructed on the hypothesis of
M. BOUGUER.

Beside the table of comparison of the arc between Greenwich and Perpignan, which I have already endeavoured to explain, this Paper is accompanied with another, which, as well as the former, was originally intended solely for my own use. With this view it was at first only computed for every five minutes of the 51st and 52d degrees of latitude, that I might thereby be enabled more readily to compare the longitudes and latitudes of the respective stations in our progress towards the coast; and more particularly to fit it for operations in the south parts of England, as likely to be first carried into execution. In order, however, to render it more generally useful, it has since been extended to every five degrees in the higher and lower parts of the quadrant, and to every single degree in intermediate latitudes. The table not only contains the degrees of the meridian and of longitude, but also those of a great circle perpendicular to the meridian, and likewise such as are oblique to it, for the other seven points of the compass. With regard to the construction of the table, it is only necessary to make
some

some remarks on that column of it, which contains the sum of the three equations, or difference between the degree of the meridian and the corresponding degree of the great circle perpendicular to it, which is the most troublesome to compute, but must be found before the degree of longitude can be obtained. The first part of M. BOUGUER's equation consists of $\frac{1}{3}$ ths of the difference between the degree of the meridian at the equator and that at the pole, *viz.* 545.12 fathoms, to be constantly added. Secondly, $\frac{1}{4}$ ths of the increment of the corresponding degree of the meridian above that at the equator, to be subtracted; and, thirdly, $\frac{1}{3}$ ths of a third proportional to the excess of the degree at the pole above that at the equator as radius, and the sine of the corresponding latitude, to be added. Now it will be found, that this last part of the equation $\frac{1}{3}$ ths, if uniformly applied, would have produced absurd results at the 75th degree of latitude, that is to say, the degrees of a great circle would there have become greater than the degree at the pole. The equation $\frac{1}{3}$ ths or $\frac{1}{4}$ th would in like manner have produced absurd results between the 79th and 80th degree. Even $\frac{1}{5}$ ths will not go on further than the 85th; and the highest equation that will go through the whole quadrant, uniformly applied, must not exceed $\frac{1}{70}$ parts of the third proportional. M. BOUGUER himself had found this, and accordingly had applied the equation with a certain modification or abatement, which nevertheless he makes no mention of in his book. Seeing, therefore, that the degrees of a great circle perpendicular to the meridian differ most from those of latitude about the tropics, I have, at the 20th degree, applied the equation $\frac{1}{3}$ ths or $\frac{1}{70}$ parts, and made the divisor increase by unity for each degree of the quadrant above that point to the pole, where it becomes $\frac{1}{70}$ parts. Below the 20th degree the

the divisor, in like manner, increases by unity to the equator, where it becomes $\frac{1}{120}$ parts. With respect to the degrees of great circles, obliquely situated to the meridian, they were chiefly intended for facilitating the computations of curvature and refraction, where, in any particular case, it might become necessary to obtain, to great precision, the relative height of a very distant station with regard to that occupied by the instrument: for in such cases, in strictness, the angle which the line makes with the meridian should be attended to.

The computations * for the construction of this table have been, as may easily be judged, sufficiently laborious; but having once begun, I was induced to go through with them, conceiving that a table of this sort would be of general utility; and that it might be very long indeed before the results of future operations, yet to be undertaken in different and very remote parts of the globe, would furnish *data* for any thing better. It must be by the united efforts of enlightened nations, conspiring as it were together in promoting the cause of science, that great objects, such as the determination of the magnitude and figure of the earth, can ultimately be obtained. Each should contribute to it, and all have it more or less in their power, according to their particular situation, and that

* Much care has been bestowed in the various computations, for the construction of the two tables appertaining to this Paper. In the last, which has been the most laborious, the differences seem to go on so uniformly, it is hoped, no error of any consequence will be found. Here it is proper that I should mention a typical *erratum* in one of the tables belonging to my Paper on the Barometer, published in the LXVII. volume of *Transactions*, for 1777. It is in Tab. VI. containing observations on heights near Carnarvon. In the column of observed height of Mercury on Moel Eilio, August 4, 1775, 1 h. 7 m. P. M. instead of 27.714 read 27.214.

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portion of its surface which has fallen to their share in the general distribution of things.

Under the auspices, and immediately under the eye, of a Sovereign who loves and cherishes the sciences, much may be done within the limits of the British islands, which, reckoning from Eastness in Suffolk to the western parts of Kerry in Ireland, contain near 12 degrees of longitude; and counting from the Channel on the south to the Orkney islands only on the north about 9 degrees of latitude. With this general purpose in view, and at the same time to lay the best foundation for the survey of the British islands, I would propose, as the operations preferably to be executed, that serieses of triangles should be extended along different meridians, particularly those of the chief Observatories, and of some remarkable hills in the central parts of the island, till they fall into the sea on the south and north; and that, across these, a great number of serieses, in the parallels likewise of the chief Observatories, and of remarkable hills or eminencies, should be extended from the east to the west coast. The *Wrekin-hill*, on the eastern skirt of Shropshire, seems to be an eligible point, being in a central situation, sufficiently conspicuous, and yet not so high as to render the frequent access to it very inconvenient. If it should be found, that longitudes may be determined by the instrument to the degree of accuracy which is expected, and that it may likewise be advantageously applied, independently of the zenith sector, in tracing any parallel whose latitude has been already ascertained, or the parallel of any remarkable point, whose meridian is known, but not its latitude; then the number of these *parallel serieses* cannot be too much multiplied: for a great many of these operations executed with due care would absolutely

absolutely give the figure of the earth, since the shortest distance from the surface to its axis would thereby be obtained.

The British dominions in the East-Indies offer a scene particularly favourable for the measurement of five degrees of latitude on the coast of Choromandel, as has been noticed by Mr. DALRYMPLE, F. R. S. in his Paper on the Marine Survey of that Coast. Two degrees of longitude, at each extremity of this arc, should likewise be measured.

The plains of Bengal, directly under the northern tropic, afford another situation where it would be of great consequence to determine the lengths of a degree or two of latitude, and as many of longitude. These two operations could not fail to be patronised by the East-India Company, who should defray the expence; since, whatever tended so much to the improvement of science in general, and so directly to that of navigation in particular, must be thought important to a Body of Merchants, whose power, as well as opulence, stand at this day unequalled in the mercantile history of the world.

But there is one operation yet to be mentioned that would contribute more than any other to the determination of the figure of the earth, which is, the accurate measurement of some degrees of longitude on the equator; because thereby the length of its semi-diameter would be immediately known, which hitherto has only been theoretically computed from the measured portions of the meridian. The Portuguese seem to be possessed of the most advantageous situation yet known on the globe for that purpose: for M. DE LA CONDAMINE has told us, that at the mouth of the river of Amazons, near the fort of *Macapa*, in three minutes north latitude, there are
extensive

extensive open plains where an operation of this sort would not have been so difficult as was at first imagined, when he proposed the scheme to the Academy of Sciences, a year before the voyage to *Quito* was thought of. Had this been adopted as the scene of their operations, instead of that elevated valley comprehended between the lofty ranges of the *Andes*, the business would have been much sooner accomplished, with infinitely less labour and fatigue, and still more satisfactorily, since degrees of longitude as well as of latitude would probably have been measured. Here then the Portuguese should determine the length of two or more degrees of longitude, and as many of latitude, by way of proof or correction of the Peruvian observations. A well conducted operation of this kind could not fail to add to the celebrity of a nation who, by their discovery of the new world in the west, and having opened the route by the Cape of Good Hope to the eastern side of the globe, may be considered as the founders of modern navigation.

With regard to operations in high northern latitudes, which would doubtless be of great importance, the Russian Empire must certainly afford a variety of situations, more or less difficult, for the purpose; and an Empress who commands so great a proportion of that region of the world, where it is said exploratory discoveries are at present carrying on by her order, can scarcely be supposed to suffer a reign, otherwise so brilliant, to expire, without directing something of this sort to be executed under the polar circle, or as near to the pole as the severity of the climate will permit, by way of confirmation or correction of the Lapland measurement, which yet stands single and by itself, without any collateral proof of its exactness. If such operations as those I have suggested towards the

close of this Paper were well executed in different parts of the world, little would then remain to be done towards the determination of the magnitude and figure of the earth, except multiplying, as much as possible, experiments with the same pendulums from the equator to very high southern and northern latitudes, that some judgement might thereby be formed of the similarity, or otherwise, of the two polar sides of the spheroid.

WILL. ROY.

Feb. 22, 1787.

E R R A T A.

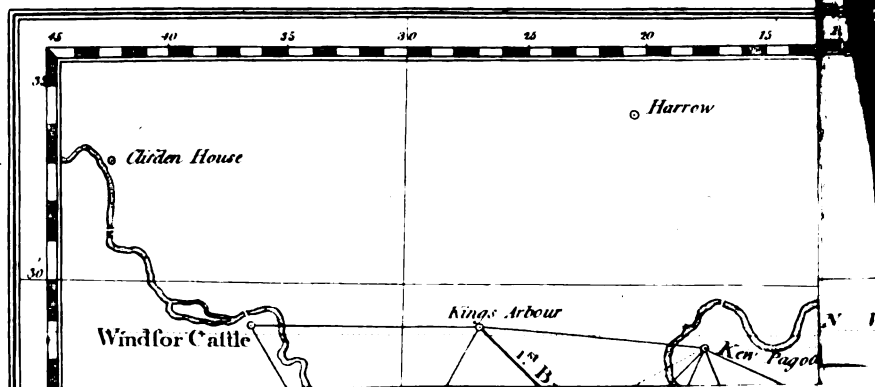
In the Second Table subjoined to this Paper, page 228, under the head "Degrees of Longitude," in the column of the length of each degree in fathoms, l. 24. from the bottom (being that which answers to the Latitude of Greenwich) for 38161.69 read 38164.00; and in the next column of differences, l. 25. from the bottom, for 53.13 read 50.82; and the next line below, for 16.12 read 18 43.

In the Map of the Triangles, for SELSBY read SELSEY.

In the First Table, p. 227. the last line of the last column but one, for 38161.7 read 38164.0.

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XX. *An Account of Three Volcanos in the Moon.* By William Herschel, LL.D. F.R.S.; communicated by Sir Joseph Banks, Bart. P. R. S.

Read April 26, 1787.

IT will be necessary to say a few words by way of introduction to the account I have to give of some appearances upon the moon, which I perceived the 19th and 20th of this month. The phænomena of nature, especially those that fall under the inspection of the astronomer, are to be viewed, not only with the usual attention to facts as they occur, but with the eye of reason and experience. In this we are however not allowed to depart from plain appearances; though their origin and signification should be indicated by the most characterising features. Thus, when we see, on the surface of the moon, a great number of elevations, from half a mile to a mile and an half in height, we are strictly intitled to call them mountains; but, when we attend to their particular shape, in which many of them resemble the craters of our volcanos, and thence argue, that they owe their origin to the same cause which has modelled many of these, we may be said to see by analogy, or with the eye of reason. Now, in this latter case, though it may be convenient, in speaking of phænomena, to use expressions that can only be justified by reasoning upon the facts themselves, it will certainly be the safest way not to neglect a full description of them, that it may appear to others

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how far we have been authorized to use the mental eye. This being premised, I may safely proceed to give my observations.

April 19, 1787, 10 h. 36' sidereal time.

I perceive three volcanos in different places of the dark part of the new moon. Two of them are either already nearly extinct, or otherwise in a state of going to break out; which perhaps may be decided next lunation. The third shews an actual eruption of fire, or luminous matter. I measured the distance of the crater from the northern limb of the moon, and found it $3' 57''$, 3. Its light is much brighter than the nucleus of the comet which M. MÈCHAIN discovered at Paris the 10th of this month.

April 20, 1787, 10 h. 2' sidereal time.

The volcano burns with greater violence than last night. I believe its diameter cannot be less than $3''$, by comparing it with that of the Georgian planet; as Jupiter was near at hand, I turned the telescope to his third satellite, and estimated the diameter of the burning part of the volcano to be equal to at least twice that of the satellite. Hence we may compute that the shining or burning matter must be above three miles in diameter. It is of an irregular round figure, and very sharply defined on the edges. The other two volcanos are much farther towards the center of the moon, and resemble large, pretty faint nebulae, that are gradually much brighter in the middle; but no well defined luminous spot can be discerned in them. These three spots are plainly to be distinguished from the rest of the marks upon the moon; for the reflection of the sun's

sun's rays from the earth is, in its present situation, sufficiently bright, with a ten-feet reflector, to shew the moon's spots, even the darkest of them: nor did I perceive any similar phænomena last lunation, though I then viewed the same places with the same instrument.

The appearance of what I have called the actual fire or eruption of a volcano, exactly resembled a small piece of burning charcoal, when it is covered by a very thin coat of white ashes, which frequently adhere to it when it has been some time ignited; and it had a degree of brightness, about as strong as that with which such a coal would be seen to glow in faint daylight.

All the adjacent parts of the volcanic mountain seemed to be faintly illuminated by the eruption, and were gradually more obscure as they lay at a greater distance from the crater.

This eruption resembled much that which I saw on the 4th of May, in the year 1783; an account of which, with many remarkable particulars relating to volcanic mountains in the moon, I shall take an early opportunity of communicating to this Society. It differed, however, considerably in magnitude and brightness; for the volcano of the year 1783, though much brighter than that which is now burning, was not nearly so large in the dimensions of its eruption: The former seen in the telescope resembled a star of the fourth magnitude as it appears to the natural eye; this, on the contrary, shews a visible disk of luminous matter, very different from the sparkling brightness of star-light.

WILLIAM HERSCHEL.

Slough near Windsor,
April 21, 1787.

P. S.

P.S. M. MÉCHAIN having favoured me with an account of the discovery of his comet, I looked for it among the Pleiades, supposing its track since the 10th of this month to lie that way; and saw it April 19th, at 10 h. 10' sidereal time, when it preceded *FL. d Pleiadum* about 54'' in time, with nearly the same declination as that star; but no great accuracy was attempted in the determination of its place. As I have mentioned the comet in a foregoing paragraph of this Paper, I thought it proper here to add my observation of it. "The comet is nearly round, with a small tail towards the north following part; the chevelure extends to about four or five minutes; and it has a central, very small, ill-defined nucleus, of no great brightness."

END OF PART I. OF VOL. LXXVII.

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PHILOSOPHICAL
TRANSACTIONS.

PART II.

L I

PHILOSOPHICAL
TRANSACTIONS,
OF THE
ROYAL SOCIETY
OF
L O N D O N.

V O L. LXXVII. For the Year 1787.

P A R T II.



L O N D O N,

SOLD BY LOCKYER DAVIS, AND PETER ELMSLY,
PRINTERS TO THE ROYAL SOCIETY.

MDCCLXXXVII.

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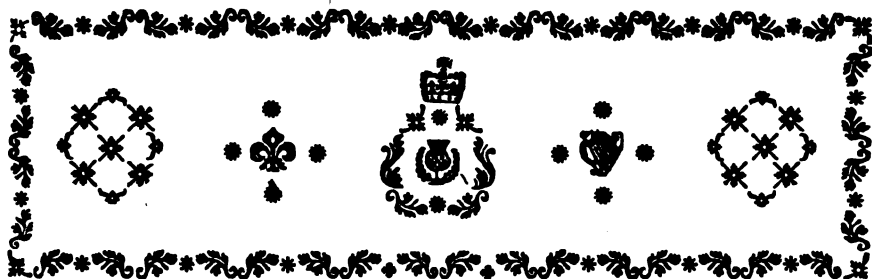
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P H I L O S O P H I C A L
T R A N S A C T I O N S.

XXI. *An Experiment to determine the Effect of extirpating one Ovarium upon the Number of Young produced.* By John Hunter, Esq. F. R. S.

Read March 22, 1787.

IN all animals of distinct sex, the females, those of the bird kind excepted, have, I believe, two ovaria, and of course the oviducts are in pairs.

By distinct sex I mean when the parts destined to the purposes of generation are of two kinds, each kind appropriated to an individual of each species, distinguished by the

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appellation of male and female, and equally necessary to the propagation of the animal; the testicles, with their appendages, constituting the male; the ovaria, and their appendages, the female sex.

As the ovaria are the organs which, on the part of the female, furnish what is necessary towards the production of the third, or young animal; and as females appear to have a limited portion of the middle stage of life allotted for that purpose; it becomes a question, whether those organs are worn out by repeated acts of propagation; or whether there is not a natural and constitutional period to that power on their part, even if such power has never been exerted? If we consider this subject in every view, taking the human species as an example, we shall discover that circumstances, either local or constitutional, may be capable of extinguishing in the female the faculty of propagation. Thus we may observe when a woman begins to breed at an early period, as at fifteen, and has her children fast, that she seldom breeds longer than the age of thirty or thirty-five; therefore we may suppose, either that the parts are then worn out, or that the breeding constitution is over. If a woman begins later, as at twenty or twenty-five, she may continue to breed to the age of forty or more; and there are, now and then, instances of women, who, not having conceived before, have had children as late in life as at fifty years or upwards. After that, few women breed, even if they should not have bred before; therefore, there must be a natural period to the power of conception. A similar stop to propagation may likewise take place in many other classes of animals, probably in the female of every class, the period varying according to circumstances; but still we are not ena-

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bled to determine, how far it depends on any particular property of the constitution, or of the ovarium alone.

As the female of most classes of animals has two ovaria, I imagined, that by removing one it might be possible to determine how far their actions were reciprocally influenced by each other, from the changes which by comparison might be observed to take place, either by the breeding period being shortened, or perhaps, in those animals whose nature it is to bring forth more than one at a time, by the number produced at each birth being diminished.

There are two views in which this subject may be considered. The first, that the ovarium, when properly employed, may be a body determined and unalterable respecting the number of young to be produced. In that case we can readily imagine, that, when one ovarium is removed, the other may produce its determined number in two different ways; one when the remaining ovarium, not influenced by the loss of the other, will produce its allotted number, and in the same time; the other, when it is affected by the loss, yet the constitution demands the same number of young each time of breeding, as if there were still two ovaria; consequently it furnishes double the number it would have been required to supply, had both been allowed to remain, but must cease from the performance of its function in half the time. The second view of the subject is by supposing, that there is not originally any fixed number which the ovarium must produce; but that the number is increased or diminished according to circumstances; that it is rather the constitution at large that determines the number; and that, if one ovarium is removed, the other will be called upon by the constitution to perform the operations of both; by which means the animal

M m 2

should

should produce, with one ovarium, the same number of young as would have been produced if both had remained.

With an intention to ascertain those points, as far as I could, I was led to make the following experiment; and for that purpose chose pigs in preference to any other animal, because they are easily managed, and breed perfectly well under the confinement necessary for experiments. Having selected two female ones of the same colour and size, and likewise a boar pig, all of the same farrow; after having removed one ovarium only from one of the females, and cut a slit in one of its ears, that there might be no mistake between it and the other, I had them well fed and kept warm, that there might be no impediment to their breeding; and whenever they farrowed, their pigs were taken away exactly at the same age.

About the beginning of the year 1779, they both took the boar; but the one which had been spayed earlier than the perfect female. The distance of time, however, was not great, and they continued breeding at nearly the same times. The spayed animal continued to breed till September 1783, when she was six years old, which was a space of more than four years. In that time she had eight farrows; but did not take the boar afterwards, and had in all seventy-six pigs. The perfect one continued breeding till December 1785, when she was about eight years old, a period of almost six years, in which time she had thirteen farrows, and had in all one hundred and sixty-two pigs; after this time she did not breed: I kept her till November 1786.

I have here annexed a table of the different times of each farrow, with the number of pigs produced.

Spayed

Spayed Sow.

Farrows..	Number of young.	Time.
1	6	Dec. 1779
2	8	July 1780
3	6	Jan. 1781
4	10	Aug. 1781
5	10	Mar. 1782
6	9	Sept. 1782
7	14	May 1783
8	13	Sept. 1783
	<hr/> 76	

November following she was put to the boar, but brought no pigs. April 1784, she was again put to the boar, without effect, and never was observed to take the boar afterwards, although often with him. November 1784, she was killed.

Perfect Sow.

Farrows.	Number of young.	Time.
1	9	
2	6	
3	8	
4	13	Dec. 1781
5	10	June 1782
6	16	Dec. 1782
7	13	June 1783
8	12	Oct. 1783
	<hr/> 87	

Eleven pigs more than were produced by the spayed sow in her eight farrows.

Farrows.

Farrows.	Number of young.	Time.
9	12	Feb. 1784
10	16	June 1784
11	12	Dec. 1784
12	16	May 1785
13	19	Dec. 1785
	<hr/>	
	75	

After which she bred no more.

The first eight farrows were	-	-	-	87
The last five farrows were	-	-	-	75
				<hr/>
Total	-	-	-	162
The number from the spayed one	-	-	-	76
				<hr/>
More than farrowed by the imperfect animal	-	-	-	86

It is observable, that both sows rather increased in their number each time the older they grew, although not uniformly; the difference between the first and last in both animals being considerable.

From the above table we find, that the sow with only one ovarium bred till she was six years old, from the latter end of 1779 till September 1783, about four years, and in that time brought forth seventy-six pigs. The perfect animal bred till she was eight years of age. In the last, if conception depended on the ovaria, it was to be expected, that she would bring forth double the number at each birth; or, if she did not, that she would continue breeding for double the time. We indeed find her producing ten more than double the number of the imperfect animal, and continuing to breed much longer.

From

From a circumstance mentioned in the course of this experiment it appears, that the desire for the male continues after the power of breeding is exhausted in the female; and therefore does not altogether depend on the powers of the ovaria to propagate, although we must at the same time allow, that it may be influenced by the existence of such parts.

If these observations should be considered as depending on a single experiment, from which alone it is not justifiable to draw conclusions, I have only to add, that the difference in the number of pigs produced by each was greater than can be justly imputed to accident, and is a circumstance certainly in favour of the universality of the principle I wished to ascertain *.

From this experiment it seems most probable, that the ovaria are from the beginning destined to produce a fixed number, beyond which they cannot go, although circumstances may tend to diminish that number; that the constitution at large has no power of giving to one ovarium the power of propagating equal to two; for, in the present experiment, the animal with one ovarium produced ten pigs less than half the number brought forth by the pig with both ovaria. But that the constitution has so far a power of influencing one ovarium, as to make it produce its number in a less time than would probably have been the case if both ovaria had been preserved, is evident from the above recited experiment.

* It may be thought by some, that I should have repeated this experiment; but an annual expence of twenty pounds for ten years, and the necessary attention to make the experiment complete, will be a sufficient reason for my not having done it.



XXII. *Experiments made to determine the positive and relative Quantities of Moisture absorbed from the Atmosphere by various Substances, under similar Circumstances. By Sir Benjamin Thompson, Knt. F. R. S.; communicated by Charles Blagden, M. D. Sec. R. S.*

Read March 22, 1787.

BEING engaged in a course of experiments, upon the conducting powers of various bodies with respect to heat, and particularly of such substances as are commonly made use of for cloathing, in order to see if I could discover any relation between the conducting powers of those substances, and their power of absorbing moisture from the atmosphere, I made the following experiments.

Having provided a quantity of each of the under mentioned substances, in a state of the most perfect cleanness and purity, I exposed them, spread out upon clean China-plates, twenty-four hours in the dry air of a very warm room (which had been heated every day for several months by a German stove), the last six hours the heat being kept up to 85° of FAHRENHEIT's thermometer; after which I entered the room with a very accurate balance, and weighed equal quantities of these various substances, as expressed in the following table.

This being done, and each substance being equally spread out upon a very clean China plate, they were removed into a very large uninhabited room upon the second floor, where they
were

were exposed 48 hours, upon a table placed in the middle of the room, the air of the room being at the temperature of 45° F. ; after which they were carefully weighed (in the room) and were found to weigh as under mentioned.

They were then removed into a very damp cellar, and placed upon a table, in the middle of a vault, where the air, which appeared by the hygrometer to be completely saturated with moisture, was at the temperature of 45° F. ; and in this situation they were suffered to remain three days and three nights, the vault being hung round, during all this time, with wet linen cloths, to render the air as damp as possible, and the door of the vault being shut.

At the end of the three days I entered the vault, with the balance, and weighed the various substances upon the spot, when they were found to weigh as is expressed in the third column of the following table.

The various substances.	Weight after being dried 24 hours in a hot room.	Weight after being exposed 48 hours in a cold, uninhabited room.	Weight after being exposed 72 hours in a damp cellar.
	Pts.	Pts.	Pts.
Sheep's wool	1000	1084	1163
Beaver's fur	1000	1072	1125
The fur of a Ruffian hare	1000	1065	1115
Eider down	1000	1067	1112
Silk { Raw, single thread	1000	1057	1107
{ Ravelings of white taffety	1000	1054	1103
Linen { Fine lint	1000	1046	1102
{ Ravelings of fine linen	1000	1044	1082
Cotton wool	1000	1043	1089
Silver wire, very fine, gilt, and flatted, being the ravelings of gold lace	1000	1000	1000

N. B. The weight made use of in these experiments was that of Cologne, the *parts* or least divisions being $= \frac{1}{81,176}$ part of a mark, consequently 1000 of these *parts* make about 52½ grains Troy.

I did not add the silver wire to the bodies above mentioned from any idea that that substance could possibly imbibe moisture from the atmosphere; but I was willing to see whether a metal, placed in air saturated with water, is not capable of receiving a small addition of weight from the moisture attracted by it, and attached to its surface; from the result of the experiment, however, it should seem that no such attraction subsists between the metal I made use of, and the watery vapour dissolved in air.

I was totally mistaken in my conjectures relative to the results of the experiments with the other substances. As linen is known to attract water with so much avidity; and as, on the contrary, wool, hair, feathers, and other like animal substances, are made wet with so much difficulty, I had little doubt but that linen would be found to attract moisture from the atmosphere with much greater force than any of those substances; and that, under similar circumstances, it would be found to contain much more water: and I was much confirmed in this opinion upon recollecting the great difference in the apparent dampness of linen and of woollen clothes, when they are both exposed to the same atmosphere. But these experiments have convinced me, that all my speculations were founded upon erroneous principles.

It should seem, that those bodies which are the most easily wet, or which receive water, in its unelastic form, with the greatest ease, are not those which in all cases attract the watery vapour dissolved in the air with the greatest force.

Perhaps

Perhaps the apparent dampness of linen, to the touch, arises more from the ease with which that substance parts with the water it contains, than from the quantity of water it actually holds: in the same manner as a body appears hot to the touch, in consequence of its parting freely with its heat, while another body, which is actually at the same temperature, but which withholds its heat with greater obstinacy, affects the sense of feeling much less violently.

It is well known, that woollen clothes, such as flannels, &c. worn next the skin, greatly promote insensible perspiration. May not this arise principally from the strong attraction which subsists between wool and the watery vapour which is continually issuing from the human body?

That it does not depend entirely upon the warmth of that covering, is clear; for the same degree of warmth, produced by wearing more cloathing of a different kind, does not produce the same effect.

The perspiration of the human body being absorbed by a covering of flannel, it is immediately distributed through the whole thickness of that substance, and by that means exposed by a very large surface to be carried off by the atmosphere; and the loss of this watery vapour, which the flannel sustains on the one side, by evaporation, being immediately restored from the other, in consequence of the strong attraction between the flannel and this vapour, the pores of the skin are disencumbered, and they are continually surrounded by a dry, warm, and salubrious atmosphere.

I am astonished, that the custom of wearing flannel next the skin should not have prevailed more universally. I am confident it would prevent a multitude of diseases; and I know of

no greater luxury than the comfortable sensation which arises from wearing it, especially after one is a little accustomed to it.

It is a mistaken notion, that it is too warm a cloathing for summer. I have worn it in the hottest climates, and in all seasons of the year, and never found the least inconvenience from it. It is the warm bath of a perspiration confined by a linen shirt, wet with sweat, which renders the summer heats of southern climates so insupportable; but flannel promotes perspiration, and favours its evaporation; and evaporation, as is well known, produces positive cold.

I first began to wear flannel, not from any knowledge which I had of its properties, but merely upon the recommendation of a very able physician (Sir RICHARD JEBB); and when I began the experiments of which I have here given an account, I little thought of discovering the physical cause of the good effects which I had experienced from it; nor had I the most distant idea of mentioning the circumstance. I shall be happy, however, if what I have said, or done, upon the subject, should induce others to make a trial of what I have so long experienced with the greatest advantage, and which, I am confident, they will find to contribute greatly to health, and consequently to all the other comforts and enjoyments of life.

I shall then think these experiments, trifling as they may appear, by far the most fortunate, and the most important ones I have ever made.

With regard to the original object of these experiments, the discovery of the relation which I thought might possibly subsist between the warmth of the substances in question, when made use of as cloathing, and their powers of attracting
I moisture

moisture from the atmosphere; or, in other words, between the quantities of water they contain, and their conducting powers with regard to heat; I could not find that these properties depended in any manner upon, or were in any way connected with, each other.

The result of my experiments upon the conducting powers of these substances, I reserve for a future communication.



XXIII. *The Principles and Illustration of an advantageous Method of arranging the Differences of Logarithms, on lines graduated for the Purpose of Computation. By Mr. William Nicholson; communicated by Sir Joseph Banks, Bart. P. R. S.*

Read March 29, 1787.

1. **I**F two geometrical series of numbers, having the same common ratio, be placed in order with the terms opposite each other; the ratio, between any term in one series and its opposite in the other, will be constant *.

2. And likewise the ratio of a term in one series to any term in the other, will be the same as obtains between any other two terms having the same relative position and distance †.

3. In all such pairs of geometrical series, as have the same common ratio, the last mentioned property obtains, though the first antecedent and consequent be taken in one pair, and the second in any other pair ‡.

$$\bullet \text{ Geom. series } \begin{cases} a & an & an^2 & an^3 & an^4 \\ b & bn & bn^2 & bn^3 & bn^4 \end{cases}$$

$$\text{Then } a : b :: an : bn :: an^2 : bn^2, \text{ \&c.}$$

$$\dagger \text{ In the foregoing series } a : bn^2 :: an^2 : bn^4 :: an : bn^3, \text{ \&c.}$$

$$\ddagger \text{ Geo. series } \begin{cases} a & an & an^2 & an^3 & an^4 \\ b & bn & bn^2 & bn^3 & bn^4 \end{cases}$$

$$\text{Geo. series } \begin{cases} d & dn & dn^2 & dn^3 & dn^4 \\ \frac{bd}{a} & \frac{bdn}{a} & \frac{bdn^2}{a} & \frac{bdn^3}{a} & \frac{bdn^4}{a} \end{cases}$$

$$\text{Then } a : bn^2 :: dn : \frac{bdn^3}{a}, \text{ \&c.}$$

4. If

4. If the differences of the logarithms of numbers be laid in order upon an arrangement of equi-distant parallel right lines, in such a manner as that a right line, drawn across the whole, shall intersect it at divisions which denote numbers in geometrical progression; then, from the condition of the arrangement and the property of this logarithmic line, it follows, first, that every right line, so drawn, will, by its intersections, indicate a geometrical series of numbers*; secondly,

* Let AB, CD, EF (Tab. X. fig. 5.) be portions of the logarithmic line, arranged according to the condition: let GH be a right line drawn across, so as to pass through points of division e, c, a , denoting numbers in geometrical progression: then will any other line IK, drawn across the arrangement, also pass through points f, d, b , denoting numbers in geometrical progression.

Demonstration. From one of the extreme points of intersection f in the last named line IK draw the right line fg , parallel to GH, and intersecting the arrangement in the points i, b ; and the ratios of the numbers $e : f, c : i$, and $a : b$, will be equal, because the intervals on the logarithmic line, or differences of the logarithms of those numbers, are equal:

$$\text{Or } \frac{e}{c} = \frac{f}{i} \text{ and } \frac{c}{a} = \frac{i}{b}.$$

$$\text{But } \frac{e}{c} = \frac{c}{a} \text{ by the condition.}$$

Therefore $\frac{f}{i} = \frac{i}{b}$; or the numbers f, i, b , are in the same continued ratio as the numbers e, c, a .

Again, the point f , the line id , and the line bb , are in arithmetical progression, and denote the differences of the logarithms of the numbers f and f, i and d, b and b .

The quotients of the numbers themselves are therefore in geometrical progression, that is,

$$\frac{f}{f} : \frac{d}{i} : \frac{b}{b}, \text{ or } \frac{i}{d} = \frac{db}{bi}.$$

$$\text{Or } \frac{i}{d} = \frac{di}{bf}, \text{ by substituting } \frac{i}{f} \text{ for its equal } \frac{b}{i}.$$

$$\text{Whence } \frac{f}{d} = \frac{d}{b} \text{ or } f : d : b. \text{ Q. E. D.}$$

that such series, as are so indicated by parallel right lines, will have the same common ratio *; and, thirdly, that the series thus indicated by two parallel right lines, supposed to move laterally without changing either their mutual distance, or parallelism to themselves, will have each the same common ratio; and in all pairs of series indicated by such two lines, the ratio between an antecedent on one parallel and the opposite term on the other, taken as a consequent, will be constant †.

5. Thus far the logarithmic line has been considered as unlimited. If, therefore, an antecedent and consequent be given, it will be possible to find both on the arrangement, and to draw two parallel lines, one over each number: and if the lines be then supposed to move, without changing either their distance or absolute direction, so that the line, which before marked an antecedent, may now mark a new antecedent; the other (by 2. and 3.) will mark a number, at the same relative position and distance, which shall be the consequent to this last antecedent after the same ratio.

6. Suppose a logarithmic line to contain no more than a single range of numbers from 1 to 10, it will not be necessary, for the purposes of computation, to repeat it; for if a slider

* In the same manner as it was proved that the line fg , parallel to GH , passes through points of division, denoting numbers in the same continued ratio as those indicated by the line GH , it may also be shewn, that the line LM parallel to any other line IK , will pass through a series of numeral points, having the same continued ratio as the series indicated by that line IK to which it is parallel.

† Because the lines preserve their parallelism to their former situation, they will indicate geometrical series having the same common ratio as before; and, because their distance measured on the logarithmic line remains unchanged; the differences of the logarithms of opposite numbers, and consequently their ratio, will be constant.

or beam have two fixed points at the distance of the interval between 1 and 10, and a moveable point be made to range between these (always to indicate the antecedent); then, I say, if the consequent fixed point fall without the rule, the other fixed point will shew the division it would have fallen on, if the rule had been prolonged. This may be easily applied to the arrangement described, N^o 4.

7. If the arrangement consist only of the logarithms from 1 to 10, and the parallel cross lines intersect that geometrical series whose successive ratios altogether, with that of the last to the first, make by composition the ratio $\frac{1}{10}$, the contrivance, N^o 6, may be applied to shew such consequents as fall, laterally, without the rule.

8. It is convenient that the arrangement of the lines be disposed so as to occupy a rectangular parallelogram; or, in other words, that the cross line, cutting the series last mentioned, may be at right angles to the length of the rule.

The construction of an instrument on the foregoing principles admits of various dispositions of the graduated lines and apparatus for measuring intervals upon them. Fig. 1. is a rule consisting of ten parallel lines, equivalent to a double line of numbers upwards of 20 feet in length. Fig. 2. is a beam compass for measuring intervals. The parts B, A, C, apply to the surface of the rule; the middle A being moveable sideways in a groove in the piece DE, so as always to preserve its parallelism to the external pieces B, C, which are fixed at a distance equal to the length of the rule, and have their edges placed according to the condition in § 7. which is here at right angles to the length. The piece DE, in the use, is applied to the edge FG of the rule. The edges or borders H, I, K, L, may

may be made of transparent horn or tortoise-shell, which are in many respects more convenient than any opaque substance.

The Use. Apply the edge of either B or C to the consequent, and slide the piece A to the antecedent, observing the difference between the numbers on the pieces denoting the lines they are found on; then, if the same edge of A be applied to any other antecedent, the other piece B or C, made use of, will intersect a consequent in the same ratio upon that line of the arrangement, which has the same situation, with respect to the antecedent, as the line of the former consequent had to its antecedent. The numbers on the pieces serve to indicate the relative situations. But if B be the consequent piece, and fall without the rule, the piece C will shew the consequent one line lower; or if C, in the like case, fall without the rule, B will shew the consequent one line higher. It would be easy to make the same kind of provision for the numbers which fall laterally without the rule: and it might be found convenient if, for the purpose of computation, instruments of this kind were to be made with an hundred or more lines. But in the present instrument, the numbers on the pieces will answer the same purpose: for if a consequent fall on a line at any given number of intervals without the rule, it will be found on that line of the arrangement, which occupies the same number of intervals, reckoned inwards from the opposite edge of the rule.

Fig. 3. is a GUNTER's scale, equivalent to that of $28\frac{1}{2}$ inches in length, published by the late Mr. ROBERTSON. It is, however, but one-fourth of the length, and contains only one-fourth of the quantity of division. In the slider GH is a moveable piece AB, across which a fine line is drawn; and there are also lines CD, EF, drawn across the slider, at a distance from each other equal to the length of the rule. The use of this is similar to that of the foregoing. The
line

line CD or EF is to be placed at the consequent, and the line in the piece AB at the antecedent: then, if the piece AB be placed at any other antecedent, the same line CD or EF will indicate its consequent in the same ratio taken the same way; that is to say, if the antecedent and the consequent lie on the same side of the slider, all other antecedents and consequents in that ratio will lie in the same manner; and the contrary if they do not, &c. But if the consequent line fall without the rule, the other fixed line on the slider will shew the consequent; but on the contrary side of the slider to that where it would else have been seen by means of the first consequent line.

Fig. 4. is an instrument equivalent to the same rule of $28\frac{1}{2}$ inches long. It consists of three concentric circles engraved and graduated on a plate of about $1\frac{1}{2}$ inch in diameter. From the center proceed two legs A, B, having right-lined edges in the direction of radii. They are moveable either singly or together. To use this instrument, place one of the edges at the antecedent, and the other at the consequent, and fix them to that angle. The two legs being then moved together, and the antecedent leg placed at any other number, the other leg will give its consequent in the like position or situation on the lines. If the line CD happen to lie between the legs, and B be the consequent leg, the number sought will be found one line farther from the center than it would otherwise have been; and, on the contrary, it will be found one line nearer in the like case, if A be the consequent leg. This instrument, differing from fig. 1. only in its circular form, and the advantages resulting from that form, the lines must be taken to succeed each other in the same manner laterally; so that numbers, which fall either without or within the arrangement of circles, will

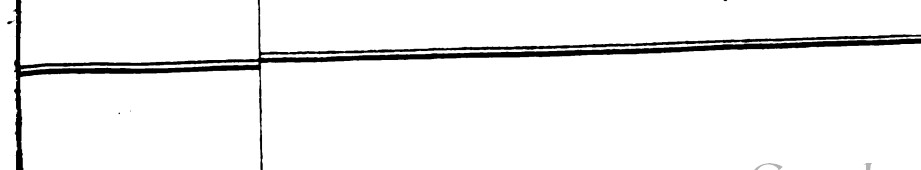
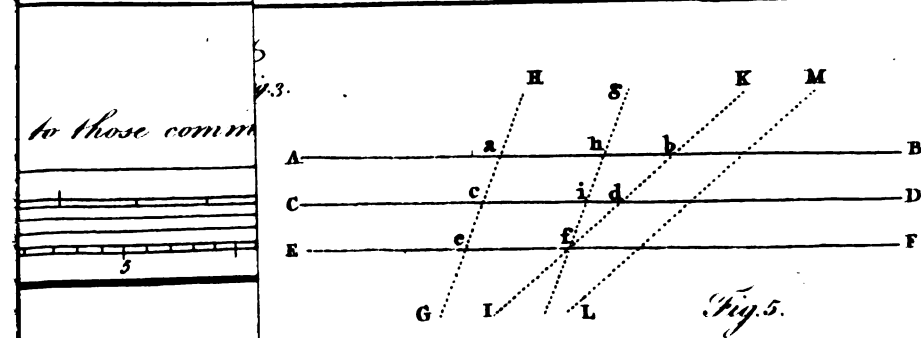
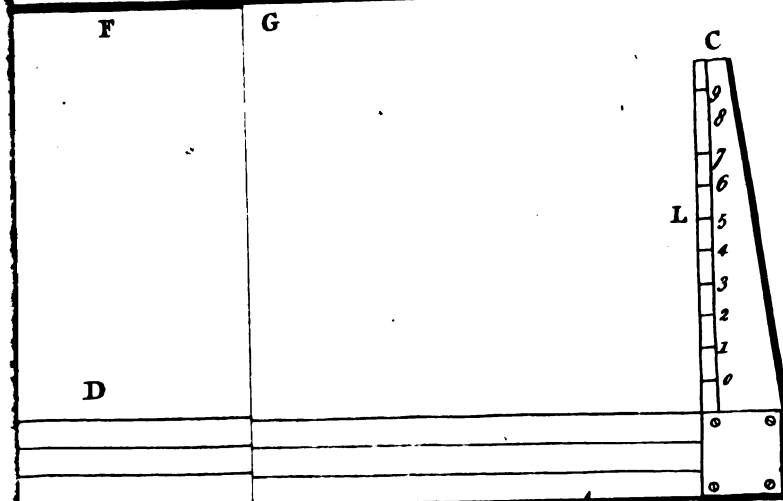
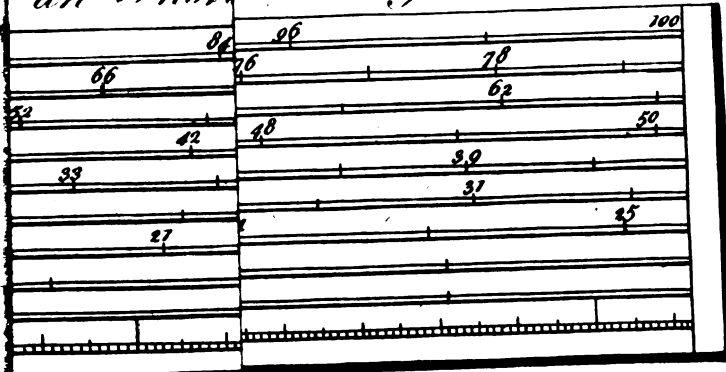
be found on such lines of the arrangement as would have occupied the vacant places if the succession of lines had been indefinitely repeated sideways.

I approve of this construction, as superior to every other which has yet occurred to me, not only in point of convenience, but likewise in the probability of being better executed, because small arcs may be graduated with very great accuracy, by divisions transferred from a larger original. The instrument, fig. 1. may be conveniently contained in a circle of about $4\frac{1}{2}$ inches diameter.

The circular instrument is a combination of the GUNTER's line and the sector, with the improvements here pointed out. The property of the sector may be useful in magnifying the differences of the logarithms in the upper part of the line of sines, the middle of the tangents, or the beginning of the versed sines. It is even possible, as mathematicians will easily conceive, to draw spirals on which graduations of parts, every where equal to each other, will shew the ratios of those lines by means of moveable radii similar to those in this instrument. But I do not, in this Discourse, propose to enter into enquiries respecting the nature of such curves, nor their utility in the present business.



Can. Instrum. Fig. 1.



XXIV. Observations tending to shew that the Wolf, Jackal, and Dog, are all of the same Species. By John Hunter, Esq. F. R. S.

Read April 26, 1787.

THE true distinction between different species of animals must ultimately, as appears to me, be gathered from their incapacity of propagating with each other an offspring capable again of continuing itself by subsequent propagations: thus the Horse and Ass beget a Mule capable of copulation, but incapable of begetting or producing offspring. If it be true, that the mule has been known to breed, which must be allowed to be an extraordinary fact, it will by no means be sufficient to determine the Horse and Ass to be of the same species; indeed, from the copulation of Mules being very frequent, and the circumstance of their breeding very rare, I should rather attribute it to a degree of monstrosity in the organs of the Mule which conceived, not being those of a mixed animal, but those of the Mare or female Ass. This is not so far-fetched an idea, when we consider that some true species produce monsters, which are a mixture of both sexes, and that many animals of distinct sex are incapable of breeding at all.

If then we find nature in its greatest perfection deviating from general principles, why may not it happen likewise in the production of Mules, so that sometimes a Mule shall breed from the circumstance of its being a monster respecting mules?

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The times of uterine gestation being the same in all the varieties of every species of animals, this circumstance becomes necessary to determine a species.

The affinity between the Fox, Wolf, Jackal, and several varieties of the Dog, in their external form and several of their properties, is so striking, that they appear to be only varieties of the same species. The Fox would seem to be a greater remove from the Dog than either the Jackal or Wolf, at least in disposition, not being either so sociable respecting its own species or man, but naturally a solitary animal; from all which I should suspect it is only allied to the Dog by being of the same genus. It is confidently asserted by many, that the Fox breeds with the Dog, but this has not been accurately ascertained; but, if it had, it would probably have been carried further, and once breeding, according to what we have said, does not constitute a species; this, however, is a part I mean to investigate. Wolves and Jackals are found in herds; and the Jackal is so little afraid of the human species, that, like a Dog, it comes into houses in search of food, more like a variety of the Dog in consequence of cultivation than chance. It is by much the most familiar of the two; for we shall find hereafter, that in its readiness to copulate with the Dog, and its familiarity with the Dog afterwards, it is somewhat different from the Wolf. The Wolf then being an animal better known in Europe, where inquiries of this kind are made, some pains has been taken to ascertain, whether or not it was of the same species with the dog; but, I believe, it has been hitherto considered as only belonging to the same genus.

Accident often does as much for natural history as premeditated plans, especially when nature is left to itself. The first instance of the Dog and Wolf breeding in this country seems

seems to have been about the year 1766. A Pomeranian Bitch of Mr. BROOKES's, in the New Road, was lined only once by a Wolf, and brought forth a litter of nine healthy puppies. The veracity of Mr. BROOKES is not to be doubted, respecting the Bitch being lined by a Wolf; yet, as it was possible she might have been lined by some common Dog without his knowledge, the fact was not clearly made out; but it has been since ascertained, that the Dog and Wolf will breed. Several noblemen and gentlemen bought some of the puppies, as I was informed by Mr. BROOKES. My Lord CLANBRASSIL purchased a Bitch-puppy; and Mr. BROOKES presented one to me, which I kept for observations and experiment. Its actions were not truly those of a Dog; it had more quickness in attending to things, was more easily startled, as if particularly apprehensive of danger, quicker in transitions from one action to another, not so ready to the call, being less docile; and from these peculiarities it lost its life, being stoned to death in the streets for a mad dog.

Hearing that Lord CLANBRASSIL's Bitch had bred, Sir JOSEPH BANKS was so obliging as, at my request, to write to his Lordship, who sent the following account.

S I R,

About seventeen or eighteen years ago, the late Lord MONTHERMER and I happened to see a Dog-wolf at Mr. BROOKES's, who deals in animals, and lives in the New Road. The animal was remarkably tame; and it struck us, for that reason, that a breed might be procured between him and a Bitch.

We promised Mr. BROOKES a good price for puppies, if he succeeded. In about a year a Bitch produced nine, and Lord

P p 2

MONT-

MONTHERMER bought one; and I had another, which was a Bitch. Lord MONTHERMER's died of fits in about two years: mine lived longer, and had puppies only once. One I gave to Lord PEMBROKE; but what became of it I do not remember. It was grand-daughter of the Wolf by the dam, and got by a large Pointer of mine.

It might be considered, that Mr. BROOKES's word was not sufficient proof that the puppies were really got by the Wolf; but the appearance of the animals, so totally different from all others of the canine species, did not leave a doubt upon our minds; and I remember HANS STANLEY, who had adopted BUFFON's opinion, was thoroughly convinced upon seeing mine. The animals had the shape of the Wolf refined: the fur long, but almost as fine as that of the black Fox.

I am afraid I have trespassed too much upon your time, and will only beg you will be assured nothing can give me more pleasure than any opportunity of assuring you how truly

I am, Sir, &c.

CLANBRASSIL.

Jan. 7, 1787.

Upon the supposition that Mr. BROOKES's Bitch was lined by no Dog but the Wolf, which I think we have no reason to doubt, the species of the Wolf is ascertained; but I chose to trace this breed still further; and hearing that Lord PEMBROKE's Bitch had likewise bred, I was anxious to know the truth of it; and, finding his lordship was in France, I took the liberty of writing to Lord HERBERT, and received in answer the following letter.

S I R,

S I R,

Wilton-house, Dec. 20, 1786.

The half-bred Wolf-bitch you allude to was given, as I have always understood, to Lord PEMBROKE by Lord CLANBRASSIL. She might, perhaps, have been bought at BROOKES's by him. She had four litters, one of ten puppies, by a Dog between a Mastiff and a Bull-dog. One of these was given to Dr. EYRE, at Wells in Somersetshire, and one to Mr. BUCKETT, at Stockbridge. The second litter was of nine puppies, some of which were sent to Ireland, but to whom I know not. This litter was by a different Dog, but of the same breed as the first. The third litter was of eight puppies, by a large Mastiff. Two of these were, I believe, sent to the present Duke of QUEENSBERRY. The fourth litter consisted of seven puppies; two of which were sent to M. CERJAT, a gentleman who now resides at Lausanne in Switzerland, and is famous for breaking dogs remarkably well. These two puppies were, however, naturally so wild and unruly, that he found it impossible to break them. She died four years ago, and the following inscription was put over the place where she is buried in this garden, by Lord PEMBROKE's orders.

Here lies Lupa,
whose grand-mother was a Wolf;
whose father and grand-father were Dogs, and whose
mother was half Wolf and half Dog. She died
on the 16th of October, 1782, aged 12 years.

I am sorry it is not in my power to give you any better account; but if you think proper to write to Lord PEMBROKE,

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who

who is at Paris, I am convinced he will be very happy to give you any further information.

I am, &c.

HERBERT.

BUFFON, whose remarks in natural history are well known, made experiments to ascertain how far the Wolf and Dog were of the same species, but without success. He says, "A She-wolf, which I kept three years, although shut up very young, and along with a Greyhound of the same age, in a spacious yard, could not be brought to agree with it, nor endure it, even when she was in heat. She was the weakest, yet the most mischievous; provoking, attacking, and biting the Dog, which at first only defended itself, but at last killed her." And in another part of his work, he makes the following observation: "The Dog, the Wolf, the Fox, and the Jackal, form a genus, of which the different species are really so nearly allied to each other, and of which the individuals resemble each other so much, particularly by the internal structure and parts of generation, that it is difficult to conceive why they do not breed together *."

This

* In the Supplement to his Works, he gives the following account which had been sent to him. "A very young She-wolf, brought up at the Marquis of SPONTIN's, at Namur, had a Dog, of nearly the same age, kept with it as a companion. For two years they were at liberty, coming and going about the apartments, the kitchen, the stables, &c. lying under the table, and upon the feet of those who sat round it. They lived in the greatest familiarity.

"The Dog was a strong Greyhound. The wolf was fed on milk for six months; after that, raw meat was given her, which she preferred to that which was dressed."

This part of natural history lay dormant till Mr. GOUGH, who sells birds and has a collection of animals on Holborn Hill, repeated the experiment on a Wolf-bitch, which was very tame, and had all the actions of a Dog under confinement. A Dog is the most proper subject for comparison, as we have opportunities of being acquainted with its dispositions and modes of expressing its sensations, which are most distinguishable in the motion of the ears and tail; such as pricking up the ears when anxious, wishing, or in expectation; depressing them when suppliant, or in fear; raising the tail in anger or love, depressing it in fear, and moving it laterally in friendship; and likewise in raising the hair on the back from many affections of the mind. This animal became in heat in the month of December 1785; and as Mr. GOUGH had some idea of breeding from wild animals, as Monkies, Leopards, &c. he was anxious to have the Wolf lined by some Dog; but she

“dressed. When she ate no one durst approach her; but at other times people
“might do as they pleased; provided they did not use her ill. At first she made
“much of all the Dogs which were brought to her; but afterward she gave the
“preference to her old companion, and from that time she became very fierce
“if any strange Dog approached her. She was lined for the first time on the
“25th of March; this was frequently repeated while her heat continued, which
“was sixteen days; and she littered the 6th of June, at eight o’clock in the
“morning; the period of gestation was therefore seventy three days at the
“most*. She brought forth four young ones of a blackish colour, some of
“whose feet, and a part of the breast, were white; in this respect taking after
“the Dog, who was black and white. From the time she littered she became
“furlly, and set up her back at those who came near her; did not know her
“masters, and would even have killed the Dog, if it had been in her power.”

* This is a longer period than in the Bitch by at least ten days; but as the account was made from the first time of her being lined, and she was in heat for a fortnight, and lined in that time, it is very probable, if the time was known when she conceived, that it would prove to be the same period as in the Dog.

would

would not allow any Dog to come near her, probably from her not being accustomed to be with Dogs, and being always chained. She was held, however, while a greyhound Dog lined her, and they were fastened together exactly as the Dog and Bitch. While in conjunction she was pretty quiet; but when at liberty, she endeavoured to fly at the Dog. In this way she was twice lined. She conceived, and brought forth four young ones. The time she went with young was not exactly known; but it was believed to be the same as in the Bitch. Two of the puppies were like the Dog in colour, who had large black spots on a white ground; one was of a black colour, and the fourth of a kind of dun, and would probably have been like the mother. She took great care of them, yet did not seem very anxious when one was taken from her by the keeper; nor did she seem afraid when strangers came into the room. Unfortunately these experiments were carried no further; one being sold to a gentleman, who carried it to the East-Indies; and the other three were killed by a Leopard, one of which I was to have had. The same Wolf was in heat in December 1786, and was lined several times by a Dog. She pupped on the 24th of February 1787, and had six puppies, which may afford opportunities, if they are thought necessary, of repeating experiments on this subject.

While pursuing this subject, I was informed, that Captain MEARS, of the Royal Bishop East-India-man, had brought home a bitch Jackal with young, which had brought forth soon after his arrival; and that he had given the bitch Jackal and one puppy to Mr. BAILEY, Bird-merchant, in Piccadilly. I went to see them, and purchased the puppy, the subject of the following experiment, which had dispositions very similar
to

to the half-bred Wolf which I had from Mr. BROOKES before mentioned.

To have a true history of this animal, I took the liberty of writing to Mr. MEARS, who politely called upon me, and, at my request, put down the particulars in the form of a letter to me, of which the following is a copy.

S I R,

I had the honour of yours the 15th instant; and with regard to the female Jackal, I can assure you, that she took a small spaniel Dog of mine on board my ship, the Royal Bishop. I had her, when a cub, at Bombay; and a very short time before I arrived in England she got to heat, and enticed this small dog into the long-boat, where I saw them repeatedly fast together. I brought her to my house in the country, where she pupped six puppies, one of which you have seen. Mr. PLAW, at N° 90, Tottenham-Court-Road, has a Dog-puppy, which will be at your service at any time you chuse to send for him, to make any further experiments: I called on Mr. PLAW, and got his promise to let you have the Dog.

I have the honour to be, Sir, &c.

WM. MEARS.

N° 107, Hatton-street,
26th Jan. 1786.

P. S. I had the Bitch on board fourteen months.

I took this puppy into the country, and chained it up near a mastiff Dog, and they were very familiar, and seemingly fond of each other. When the Bitch became first in heat, I could not get a proper Dog for her; but the latter end of September being again in the same situation, several Dogs were procured, and left with her. They appeared indifferent about her, probably from being in a strange place; and she did not seem inclined to be familiar with them; whether the great Dog might be able to line her I do not know; she was, however, twice tied by a Tarrier on the 3d of October. In a few weeks she was evidently become bigger; and on the 30th of November, in all fifty-nine days, she brought forth five puppies. Some days before this period she dug a hole under ground, by the side of her kennel, in which she brought forth, and it was some time before she would allow the puppies to stay in the kennel when put there. In about eight days some, and nine days others of them began to open their eyelids.

Here then is an absolute proof of the Jackal being a Dog; and it appears to me, that the Wolf is equally made out to be of the same species. It now then becomes a question, whether the Wolf is from the Jackal, or the Jackal from the Wolf, supposing they had but one origin? From the supposition, that varieties become more tame in their nature, we should be led to believe, the Wolf to be the original, and that the Jackal was a step towards civilisation in that species of animal. There are Wolves of various kinds, each country having a Wolf peculiar to itself; but the Jackals that I have seen have been more uniformly the same, both those from Africa, and those from the East-Indies. I am informed, however, that they vary in size. Whether all the Wolves of different countries are of one species, or some of them only of the same

same genus, I do not know ; but I should rather suppose them to be all of one species. What is with me an argument in favour of this supposition is, that, if there were Wolves of distinct species, we should have had by this time a great variety of that species of Wolves, with the various dispositions arising from variation in other respects ; and those varieties now turned to very useful purposes, as has been the case with the Dog ; for all the Wolves we are yet acquainted with, have naturally the principle of cultivation in them, as much probably as any animal, or as much at least as those Wolves we now know to be Dogs. The not having a civilised species of Wolf is, indeed, with me a proof that they are all of the same species with the Dog. If they are all of the same species with the Dog, then the first variety that took place was still in the character of a Wolf, differing only in colour, or some trivial circumstance, which could only take place from a difference in climate ; civilisation or cultivation in a state of nature being the same in them all. Where they became Jackal, or what we now call Dog, is difficult to say ; or what Dog we can call the first remove, as many Dogs differ very much from one another ; or whether the Jackal is the intermediate link between the Wolf and the Dog. In either case we have three great varieties in this species, Wolf, Jackal, and Dog, with the varieties in each. If the Dog is proved to be the Wolf tamed, the Jackal may probably be the Dog returned to his wild state.

To ascertain the original animal of a species, it is proper to examine all the varieties of that species, and see how far they have the character of the genus, and what resemblance they bear to the other species of the genus ; for it is natural to suppose, that the original, or the animal which is nearest to it,

will have more of the true character of the genus, and will have a stronger resemblance to the species nearest allied to it, than any of the other varieties of its own species.

If we apply this to the Dog, and consider the Fox as a distinct species, which there is great reason to believe it is, that variety which has the strongest resemblance to the Fox, is to be looked upon as the original of all the others ; which will prove to be the Wolf.

Another mode of considering this subject, which is however secondary to the above, is, supposing that all animals were at first wild ; and, therefore, that those animals which remain wild, are the original stock ; and that the further we find animals removed from their originals in appearance, they are really further removed in consequence of variation taking place from cultivation, so that we may still be able to trace the gradation. What gives some force to this idea is, that where the Dogs have been least cultivated, there they still retain most of their original character, or similarity to the Wolf or the Jackal, both in shape and disposition. Thus the shepherd's Dog, all over the world, has strongly the character of the Wolf or Jackal ; so that but little difference is to be observed, except in size and hair. Size is, perhaps, a variety taking place under a variety of circumstances ; but difference in hair is, in general, influenced by climate, although perhaps not always so. Thus the Wolf has longer and softer hair than the Jackal, because he is a more northern animal ; and the Jackal and shepherd's Dog in Portugal and Spain have shorter and stronger hair than those of Germany or Kamchatka, from inhabiting warmer climates. But when we consider their general shape, the character of countenance, the quick manner with the pricked and erect ears, we must suppose them varieties of
of

of the same species. The smelling at the tail has been described as characteristic of the Dog; but, I believe, it is common to most animals, and only marks the male; for it is the most certain way the male has of knowing the female, and also discloses another scent, which is the final intention, whether the female is disposed to receive the male.

The Esquimaux Dog, and that found among the Indians as far south as the Cherokees; the shepherd's Dog in Germany, called Pomeranian; the shepherd's Dog in Portugal and Spain; have all a strong similarity to the Wolf and Jackal.

BUFFON, on the origin of Dogs, seems to have possessed nearly the same idea; for he says the shepherd's Dog is the original stock from which the different races of Dogs have sprung.

As the Wolf turns out to be a Dog, it seems astonishing, that there was no account of Dogs being found in America. But this I consider as a defect in the first history of that country, for there are Wolves; and I think, in spite of all that has been said to the contrary, the Esquimaux and Indian Dog is only a variety from a Wolf in that country, which had been tamed. Mr. CAMERON, of Titchfield-street, who was many years among the Cherokees, and considerably to the westward of that country, observes, that the Dog found there is very similar to the Wolf; and that the natives consider it to be a species of tame Wolf; but as we come more among the Europeans who have settled there, the Dogs are more of a mixed breed; for why they should only have had this kind of Dog transported among them, while every other part of America has the varieties of Europe, is not easily solved.

The voice of animals is commonly characteristic of the species; but I should suppose, it is only characteristic of the original species, and not always of the variety, and this

supposition

supposition holds good in the Dog species. It would appear, that the voice of the Wolf and the Jackal is very similar, and is principally conveyed through the nose, and exactly resembles that noise in Dogs, which is a mark of longing or melancholy, and also of fondness; but has no resemblance to the bark of the Dog, which they do not perform. Barking is peculiar to certain varieties of the Dog kind, and even some that do bark, do it less than others. The Dogs in the South-Sea islands do not bark: our Greyhound barks but little; while the Mastiff, and many of the smaller tribe, as Spaniels, are particularly noisy in this way. It would appear as if the frequency of this noise arose from imitation; for the Dogs in the South-Seas learn to bark; and others, as the Hound, have a peculiar howl, which, by huntsmen, is called the tongue. This noise, as also the bark, is made by opening the mouth. A variety in the voice, or some parts of the voice, in the varieties of the same species, is not peculiar to the Dog.



XXV. *Experiments on the Congelation of the Vitriolic Acid.* By James Keir, *Esq. F. R. S.*; communicated by Henry Cavendish, *Esq. F. R. S.*

Read May 3, 1787.

THAT the vitriolic acid sometimes assumes a solid, crystalline state, has been observed by BASIL VALENTINE, and by many later chemists; but their relations of this appearance are neither sufficiently explicit, with regard to the essential and concomitant circumstances, nor do they seem very consistent with each other. It appears, however, that two very distinct species of congelation of this acid have been noticed. That which is described by the older chemists, and also by some modern authors, requires no greater degree of cold than the common temperature of the air, even in summer, and is peculiar to that acid which is obtained by distillation from martial vitriol, and which is possessed of a smoking quality in a high degree: for not only the authors, by whom this congelation has been observed, have given this description of the acid employed, but also the late experiments of M. DOLLEFUS * seem to shew, that the smoking quality is essential to the phænomenon; for neither the acid obtained from vitriol, when deprived by rectification of its smoking quality, nor the English oil of vitriol (which is known to be obtained by burning sulphur, and

* Crell Annalen 1785.

which

which does not smoke), were found, by his trials, to be susceptible of this species of congelation. The acid, thus congealed, has been called *glacial*, or *icy oil of vitriol*.

The other kind of congelation has been little noticed till lately. To this congelation every kind of vitriolic acid is subject, whether it smokes or not, and whether it has been prepared from martial vitriol, or from sulphur, provided the cold to which it is exposed be sufficiently intense: for the cold, requisite for this species of congelation, is considerably greater than what is sufficient for the former.

Mr. MACQUER relates, in the second edition of his Dictionary of Chemistry *, that the Duke D'AYEN had observed the congelation of concentrated vitriolic acid, which had been exposed to a cold expressed by 13 or 14 degrees below 0 of REAUMUR's scale; but that mixtures, consisting of one part of the above-mentioned concentrated acid, with two or more parts of water, could not be frozen by the cold to which he exposed them, till he had diluted the acid so much, that its density was to that of water as 104½ to 96; in which latter case of congelation, it is probable, that the water only did freeze, as it does in dilute solutions of neutral salts. M. DE MORVEAU † has made similar experiments, with a view to verify those of the Duke D'AYEN, and with similar success. By means of an intense cold, produced by adding spirit of nitre to pounded ice, he congealed a part of some vitriolic acid, which he had previously concentrated. He observed, that although a very intense cold had been employed to freeze the concentrated acid, it nevertheless remained congealed in much less degrees of cold, and that it thawed very slowly. Lastly, some experiments

* Article, Vitriolic Acid.

† Mem. de l'Acad. de Dijon, pour 1782.

have

have lately been made by Mr. M^c NAB, at Hudson's Bay, on the congelation of acids by intense cold; an account of which experiments is given in the Philosophical Transactions for 1786, by Mr. CAVENDISH, at whose desire they had been made. These experiments are the more valuable, as the density of the acids employed, and the temperature, and other concomitant circumstances, have been distinctly noted; and they are rendered still more interesting, by the very judicious remarks made on them by Mr. CAVENDISH. It is there related, that a vitriolic acid, whose specific gravity was to that of water as 1843.7 to 1000, froze when exposed to a cold of -15° of FAHRENHEIT's scale; that another more dilute vitriolic acid, consisting of 629 parts of the former concentrated acid, and 351 parts of water, congealed in a temperature of -36° ; and that when the acid was further diluted, it was found capable of sustaining a much greater cold without freezing. In these experiments, as also in those of M. DE MORVEAU, it appeared, that the whole of the acid did not congeal, but that part of it retained its fluidity. Mr. CAVENDISH found, on examining the part which had congealed, and that which had remained fluid, that they were *nearly* of the same strength; and he is thence led to think, that the difference between them, by which the one is more disposed to congeal than the other, does not depend on their different strengths, but on some quality less obvious, and the same which constitutes the difference between glacial and common oil of vitriol. In all the experiments which had been made by the Duke d'AYEN, M. DE MORVEAU, and Mr. M^c NAB, the vitriolic acid, when strong, had frozen with less cold than when diluted; but these experiments did not enable Mr. CAVENDISH to determine, whether this acid has any determinate strength or *point of easiest freezing*

(such as he had discovered to be possessed by spirit of nitre), or whether the cold requisite for congelation does not continually diminish, as the strength of the acid increases, without limitation. This latter opinion he thinks the most probable, from the circumstance of the Duke d'AYEN's and M. DE MORVEAU's acids having frozen with a considerably less intense cold than those of Mr. M^c NAB, which, he supposes, were weaker, as the former acids had been concentrated purposely.

The observations which I have made, and am going to relate, apply solely to the latter kind of congelation of the vitriolic acid, as the acid which I employed was of the kind that is prepared by burning sulphur, and is commonly sold in England under the name of oil of vitriol, and was perfectly free from colour, smell, or smoking quality. After a severe frost at the end of the year 1784, and beginning of 1785, I observed that some vitriolic acid, contained in a corked phial, had congealed; while other parcels of the same acid, some stronger and some weaker, equally exposed to the cold, had remained fluid. As I imputed the congelation to the great intensity of the cold, I was afterwards much surprised, when the frost ceased, to find that the acid remained frozen during many days, when the temperature of the air was sometimes above 40° of FAHRENHEIT's scale; and when the congealed acid was brought into a warm room, purposely to thaw it, a thermometer, placed in contact with it during its thawing, continued stationary at 45°. From these circumstances I concluded, that the freezing and thawing point of this acid was very near the last mentioned degree; and, accordingly, upon exposing the liquor which had been thawed to the air, at the temperature of 30°, the congelation again took place in a few hours. From the circumstance of other parcels of the same acid, but of
4 different

different strengths, remaining fluid, although they had been exposed to a much greater cold than was necessary for the congelation of that acid liquor which had frozen, I was led to believe, that there must be some certain strength at which the vitriolic acid was more disposed to freeze than at any other, greater or less. I knew that the specific gravity of the acid which had frozen was nearly to that of water as 1800 to 1000, and that of the stronger acid, which had not frozen, was as 1846 to 1000; which last is the usual density of the oil of vitriol commonly sold in England. I knew also, that the acid which had frozen was in no respect but in strength different from the stronger acid which had retained its fluidity; having myself, some weeks before, taken the former acid from the bottle containing the latter, and diluted it with water till it was reduced to the specific gravity of 1800.

Although from the above observations I was convinced of the proposition generally, that the vitriolic acid is most disposed to freeze when at a certain strength, and then it is susceptible of congelation by means of much less cold than has been hitherto imagined; yet, as only part of my acid had frozen, I could not with certainty know the strength of the frozen part, and I therefore was not able to state, with any accuracy, the degree of strength most favourable to congelation, nor the limits of strength within which the acid may be congealed by such moderate cold. In the following winter I had not leisure to pursue the subject; but since the commencement of the present year, I have verified my former observation with more attention to the exact densities of the acids; and I have found, that the point of strength most favourable to congelation is very determinate, and that a very small variation above or below that point renders the acid incapable of freezing without a considerable augmentation of cold. As the

R r 2

acid,

acid, when brought to the proper strength, was capable of freezing with less cold than water does, I immersed several acids of different strengths in melting snow, instead of exposing them to the air, the temperature of which was variable, whereas that of melting snow was constant and determinate. Those acids which would not freeze in melting snow, were afterwards immersed in a mixture of snow, water, and common salt, the temperature of which was not so constant and determinate as that of melting snow; but it generally remained for several hours at about 18° , and was sometimes several degrees lower. The intention of adding water to the snow and salt was to lessen the intensity of the cold of this mixture, and to render it more permanent than if the snow and salt alone were mixed.

The acids which had frozen in melting snow, and which were five in number, having been thawed and brought to the temperature of 60° , were found on examination to have the following specific gravities.

1786

1784

1780

1778

1775

Those acids which would not freeze in melting snow, but which froze when immersed in snow, water, and salt, having been exposed to a greater cold, were of a greater latitude of density. Their specific gravities, when brought to the temperature of 60° , were found to be expressed by the following numbers.

1814

1810

1804

1804

1794

1790

1770

1759

1750

The acids which remained, and which would not freeze either in melting snow, or in the mixture of snow, salt, and water, were found on examination to have the following specific gravities.

1846

1839

1815

1745

1720

1700

1610

1551

It appears, from the first table of specific gravities, that the medium density of the acids which did freeze with the cold of melting snow was 1780; and from the second table it appears, that, at the densities of 1790 and 1770, the acid had been incapable of freezing with that degree of cold. Hence it follows, that 1780 is nearly the strength or density of easiest freezing; and that an increase or diminution of that density, equal to $\frac{1}{178}$ th part, renders the acid incapable of freezing with the cold of melting snow, notwithstanding this cold is some degrees above the freezing point of the most congelable acid. From the second table of specific gravities it appears, that by applying a more intense cold, namely, that produced by a mixture of snow, salt, and water, the limits of the density of the acids capable of congelation were extended to about $\frac{2}{178}$ above or below the point of easiest freezing: and there seems .

seems little reason to doubt, that, by greater augmentations of cold, these limits may be further extended; but in what ratio these augmentations and extensions proceed cannot be determined without many observations made in different temperatures.

Although it is probable, that the most concentrated acids may be frozen, provided the cold be sufficiently intense, yet there seems to me reason to believe, that some of the congelations which have been observed in highly concentrated acids have been effected in consequence of the density of these acids having been reduced nearly to the point of easy freezing by their having absorbed moisture from the air: for the Duke D'AYEN and M. DE MORVEAU exposed their acids to the air, in cups or open vessels; and the latter author even acquaints us, that, on examining the specific gravity of the acid which had frozen, he found it to be to that of water as 129 to 74; which density being less than the point of easiest freezing, proves that the acid which he employed, and which he had previously concentrated, had actually been weakened during the experiment. I have several times exposed concentrated oil of vitriol in open vessels in frosty weather; and I have sometimes, but not always, observed a congelation take place. Upon separating the fluid from the congealed part, and upon examining the specific gravity of the latter, after it had thawed, I found that it had been reduced to the standard of easiest freezing. When the congealed acid was kept longer exposed, it gradually thawed, even when the cold of the air increased; the reason of which is not to be imputed to the heat produced by the moisture of the air mixing with the acid, for this cause operated during the congelation, but principally to the diminution of density below the point of easy freezing, which was occasioned by the continued absorption of moisture from the air, and which rendered

rendered the acid incapable of continuing frozen without a great increase of cold.

It appears then, that the concentration of M. DE MORVEAU's acid, at the time of its congelation, from which circumstance Mr. CAVENDISH infers generally, that the vitriolic acid freezes more easily as it is more dense, is not a true premise; and that therefore the inference, though justly deduced, is invalid. On the contrary, there seems every reason to believe, from the analogy of my experiments above mentioned, that as the density of the acid increases beyond the point of easiest freezing, the facility of the congelation diminishes; at least, to as great density as we have been ever able to obtain the vitriolic acid; for if it were possible to divest it intirely of water, it would probably assume a solid state in any temperature of the air.

The crystallization of the frozen vitriolic acid is more or less distinct, according to the slowness of its formation, and other favourable circumstances. Sometimes the crystals are very distinctly shaped, large, and very hard. Their form is the same as the common form of mineral alkali and of selenitic spar, but with angles different in dimensions from either of these. They are solids consisting of ten faces, of which the two largest are equal, parallel, and opposite to each other, and are oblique-angled parallelograms or rhomboids, whose angles are, as near as I could measure them, of 105 and 75 degrees. Between these two rhomboidal faces are placed eight faces of the form of trapeziums. Thus each crystal may be supposed to be composed of two equal and similar frustums of pyramids joined together by their rhomboidal bases. I observed, that the crystals always sunk in the fluid acid to the bottom of the vessel, which shewed that their density was increased by congelation.

I thought

I thought of ascertaining their specific gravity by adding gradually to this fluid part some concentrated vitriolic acid, till the crystals should float in the liquor, the examination of whose specific gravity would determine that of the floating crystals. But I was surprised to find, that the crystals sunk even in the concentrated acid, and consequently were denser. I then poured some of the congelable acid, previously brought to the freezing temperature, into a graduated narrow cylindrical glass, up to a certain mark, which indicated a space equal to that occupied by 200 grains of water. The glass was placed in a mixture of snow, salt, and water, and when the acid was frozen, a mark was made on the part of the glass to which the acid had sunk. Having thawed the acid, and emptied the glass, I filled it with water to the mark to which the acid had sunk by freezing, and I found, that fifteen grains more of water were required to raise it to the mark expressing 200 grains; which shews, that the diminution of bulk, sustained by the acid in freezing, had been equal to $\frac{1}{13.3}$ of the whole.

Computing from this *datum*, we should estimate the specific gravity of the congealed acid to have been 1924; but as it contained evidently a great number of bubbles, its real specific gravity must be considerably greater than the above determination, and cannot easily be ascertained on account of these bubbles. By way of comparison, I observed the alteration of bulk which water contained in the same cylindrical glass would suffer by freezing; and I found that its expansion was equal to about $\frac{1}{10}$ th part of its bulk. The water had been previously boiled; but it nevertheless contained numberless bubbles. In this respect then there is a remarkable difference between the congelations of water and of vitriolic acid; but, perhaps, the difference

difference arises principally from the bubbles of elastic fluid, which may be in greater quantity, and may add more to the bulk of the water than of the acid.

Greater cold is produced by mixing snow or pounded ice with the congealed than with the fluid acid, but the quantity I have not determined. There is reason to believe it may be considerable. In the experiments made at Hudson's Bay, by Mr. Mc NAB, the greatest cold which he had produced by mixing acids with snow, was effected by a vitriolic acid which had previously congealed; and to this circumstance of the congelation of the acid, Mr. CAVENDISH justly imputes the intensity of the cold, as the liquefaction of both the frozen acid and the snow had concurred towards this effect; whereas, in mixing fluid acids with snow, the thawing of the snow is probably the sole productive cause.

I was desirous of comparing the times required for the liquefaction of ice and of congealed acid, when both were exposed to the same temperature. For this purpose I filled two equal and similar cylindrical glasses; one with the congelable vitriolic acid, and the other with water; and, after having immersed them in a freezing mixture till both fluids were frozen, and reduced to the temperature of 28° , I withdrew the glasses from the freezing mixture, wiped them dry, and placed them together in a room, where the thermometer stood at 62° . In 40 minutes the ice was thawed, and in 95 minutes the acid was liquefied, at the end of which time the thermometer, which stood near the glasses, had risen to 64° . It appears then, that the congealed acid requires more than twice the time for its liquefaction, when exposed to that temperature, than ice does; but I do not think that we can infer, that the heats absorbed and rendered *latent*, as some late philosophers express them-

selves; or, in other words, that the cold generated by the liquefaction of ice and of congealed acid are in the above proportion of the times, from the following consideration; that, as during the liquefaction of the ice, its temperature remains stationary at 32° , and during the liquefaction of the acid, its temperature remains about 44° or 45° , the ice, being considerably colder than the acid, will take the heat from the contiguous air much faster.

The experiment does however shew, that a considerable quantity of cold is generated by the liquefaction of this acid; and hence it appears probable, that in making experiments of producing cold artificially, by mixing snow with acids in very cold temperatures, it would probably be useful to employ a vitriolic acid of the proper density for congelation, and to freeze it previously to its mixture with snow.

It must not, however, be imagined, that the cold generated by the mixture of these two frozen substances is nearly equal to the sums of the colds generated by the separate liquefactions of the congealed acid and ice, when singly exposed to a thawing temperature: for the mixture resulting from the liquefaction, consisting of the vitriolic acid and the water of the snow, appears, from the generation of heat which occurs in the mixture of these ingredients in a fluid state, to be subject to different laws relatively to heat, than either of the ingredients separately. And the heat, thus generated, as soon as the congealed acid and ice are brought to a fluid state, must counteract, in some measure, the cold produced by the liquefaction.

The vitriolic acid, like water and other fluids, is capable of retaining its fluidity when cooled considerably below its freezing point. I placed a phial, containing some congelable vitriolic

vitriolic acid, in a mixture of salt, snow, and water ; and soon afterwards, while the acid was yet fluid, I immersed in it a thermometer, the mercury of which quickly sunk from 50° to 29° . While I was moving the thermometer in the fluid, in order to make it acquire the exact temperature, I saw the mercury suddenly rise, and upon looking at the acid, I observed numberless small crystals floating in it, which had been suddenly formed. The degree to which the mercury then rose was $46^{\circ}\frac{1}{4}$. Another time, while the acid was freezing, the thermometer placed in it stood at 45° .

From the above observations, the following inferences may be drawn.

1st, That the vitriolic acid has *a point of easiest freezing*; that is, there is a certain strength or density, at which this acid freezes with considerably less cold than at any other strength, greater or less ; and that this density is nearly to that of water as 1780 is to 1000.

2dly, That the greater or less disposition of congelation of the vitriolic acid, which is free from the smoking quality that is peculiar to the acid obtained by distilling martial vitriol, does not depend on any other quality or circumstance than its strength or density.

3dly, That the freezing and thawing degree of the most congelable acid is about 45° of FAHRENHEIT'S scale. It is, however, to be observed, that this degree is inferred from the temperature indicated by the thermometers immersed in the freezing and thawing acids ; but that I never effected the congelation of the fluid acid, without exposing it to a greater cold, namely, either that of melting snow, or of the external air in frosty weather.

S f 2

Like

Like water, this acid possesses the property of retaining its fluidity when cooled several degrees below its freezing point; and of rising suddenly to this point, when its congelation is promoted by agitation, or by contact with even a warmer thermometer.

4thly, That, like water and other congelable fluids, the vitriolic acid generates cold during its liquefaction, and heat during its congelation; the quantity of which heat and cold, so generated, remains to be determined by future experiments.

5thly, That the acid, by congelation, when the circumstances for distinct crystallization are favourable, assumes a regular crystalline form, a considerable solidity and hardness, and a density much greater than it possessed in a fluid state.

With respect to the first mentioned species of congelation, which is peculiar to the smoking vitriolic acid that is procured from martial vitriol, although I have had no opportunity of seeing it, as all the vitriolic acid, that is used in this country, is obtained by burning sulphur, yet I will beg leave to suggest, that it may be worth the attention of those chemists to whom it occurs, to observe more accurately than has been done, the freezing temperature and the density of the congelable acids; and to examine whether the density of this smoking acid also is connected with the glacial property. It seems further to be deserving of investigation, whether there is not some analogy between the congelation of the smoking oil of vitriol, and the very curious crystallization which Dr. PRIESTLEY observed in a concentrated vitriolic acid, saturated with nitrous acid vapours *; and whether this smoking quality does

* Experiments and Observations relating to various Branches of Natural Philosophy,

does not proceed from some marine or other volatile acid, which may be contained in the martial vitriol, whence the vitriolic acid is obtained.

Philosophy, vol. I. p. 26. and 450. M. CORNETTE has also effected the crystallization of vitriolic acid by distilling it with nitrous acid and charcoal. *Memoir. de l'Acad. des Scienc. Paris, pour 1779.*



XXVI. *An Account of some new Experiments on the Production of artificial Cold. In a Letter from Thomas Beddoes, M. D. to Sir Joseph Banks, Bart. P. R. S.*

Read May 10, 1787.

DEAR SIR,

Oxford, May 2, 1787.

MR. WALKER, Apothecary to the Radcliffe Infirmary here, has been engaged upwards of a year in a series of experiments on the means of producing artificial cold, several of which seem to me to be very remarkable, and such as, considering their novelty, and the attention which has lately been paid to this subject, I flatter myself, will be found to deserve a place among the Transactions of the Society over which you preside.

Mr. WALKER, in his first experiments, found, as BOERHAAVE had done before him, that sal ammoniac, as well as nitre, well dried in a crucible, and reduced to a fine powder, will produce a greater degree of cold than if they had not received this treatment. But BOERHAAVE, by sal ammoniac, lowered the temperature of water only by 28° ; whereas Mr. WALKER observed his thermometer to fall 32° , and when he used nitre 19° . It occurred to him, that the combination of these substances would produce a greater effect than either separately: and he found that this was really the case. A proposal for freezing water in summer, mentioned by Dr. WATSON (Essays, III. 139.) determined him to attempt the same thing

thing in this way. Accordingly, April 28, 1786, the thermometer standing at 47° , he made a solution of a powder, consisting of equal parts of sal ammoniac and nitre, in a basin, by means of which he cooled some water, contained in a glass tumbler, to 22° . To this he added some of the same powder, and immersed two very small phials in it; one containing boiled, the other unboiled water; when he soon found the water in the phials to be frozen, the unboiled freezing first.

Having observed that GLAUBER's salt, when it retains its water of crystallization, produces cold during its solution, he thought of adding this to his other powers, and July 18, 1786, reduced the thermometer 46 degrees. In this experiment the following proportions were used: the temperature of the air being 65° , to water four ounces, at 63° , were added,

Of sal ammoniac	℥ xi	thermometer sunk to 32° , that is, 31°
Of nitre	℥ x	- - - 24° , that is, 8°
Of GLAUBER's salts	℥ ij	- - - 17° , that is, 7°
		<hr/> 46°

In this way he froze water on a day so hot that the thermometer in the shade stood at 70° . By first cooling the salts and water in one mixture, and then making another of these cooled materials, he sunk the thermometer 64 degrees.

August 28. The temperature of the air being 65° , half an ounce of rectified spirit of wine was diluted with three ounces and an half of water, and immersed in the same frigorific mixture. When cooled to 24° , it began to freeze. A quantity of the neutral salts, likewise cooled in the mixture, were put into the diluted spirit, when the thermometer fell to -4° , so that the liquor was cooled 69 degrees.

Spirit of nitre, diluted in the manner described by Mr. CAVENDISH (Phil. Transf. vol. LXXVI. part I.), having reduced the thermometer to -3° , sal ammoniac was added, upon which it fell to -15° .

Nitrated volatile alkali, during its solution in water, reduced the thermometer 35 degrees (from 50° to 15°); but the cold was not increased by sal ammoniac or nitre.

Mr. WALKER's most remarkable experiment was made on the 21st of March, 1787, when he found that nitrous acid, when poured upon GLAUBER's salt, produced effects nearly the same as when it is poured on pounded ice; and that the cold, thus produced, is rendered still more intense by the addition of sal ammoniac in powder.

Mr. WALKER, by many trials, discovered that the best proportion of these ingredients is the following: of concentrated nitrous acid, 2 parts by weight, of water 1 part; of this mixture cooled to the temperature of the atmosphere eighteen ounces, of GLAUBER's salt a pound and an half (avoir-dupois), and of sal ammoniac twelve ounces. On adding the GLAUBER's salt to the nitrous acid, thus diluted, the thermometer fell from $+51^{\circ}$ to -1° , or 52 degrees; and on adding the sal ammoniac it fell to -9° , that is full 60 degrees. Nitrated volatile alkali, employed instead of sal ammoniac, produced a cold rather more intense.

By means of this mixture, in a very few minutes, in the laboratory before the class, I froze some spirits above proof, diluted with an equal bulk of water; and another gentleman this day sunk the thermometer 68 degrees.

On April 20, 1787, Mr. WALKER effected the congelation of quicksilver by a combination of these mixtures, without a particle of snow or ice. When he began his experiment the
temperature

temperature of the mercury was 45° , so that, the freezing point of that metal being -39° , there were produced 84 degrees of cold.

This experiment was performed as follows. Four pans, of sizes progressively diminishing, so that one might be placed within the other, were procured. The largest of these pans was placed in another vessel still larger, in which the materials for the second frigorific mixture were thinly spread, in order to be cooled. The second pan, containing the liquor (*viz.* vitriolic acid, properly diluted) was placed in the largest pan. The third pan, containing the salts for the third mixture, was immersed in the liquor of the second pan; and the liquor for the third mixture was put into wide-mouthed phials, which were immersed in the second pan likewise, and floated round the third pan. The fourth pan, which was the smallest of all, containing its cooling materials, was placed in the midst of the salts of the third pan.

Of the materials for the mixtures to be made in these four pans, the first and second consisted of diluted vitriolic acid and GLAUBER'S salt, the third and fourth of diluted nitrous acid, GLAUBER'S salt and sal ammoniac, in the proportions assigned.

The pans being adjusted in the manner above described, the materials of the first and largest pan were mixed: this mixture reduced the thermometer to $+10$, and cooled the liquor in the second pan to $+20$; and the salts for the second mixture, which were placed underneath in the large vessel, nearly as much. The second mixture was then made with the materials thus cooled, and it reduced the thermometer to 3° . The ingredients of the third mixture, by immersion in this, were cooled to $+10^{\circ}$, and when mixed reduced the thermometer to -15° . The materials for the fourth mixture were cooled by immersion in this third mixture to about -12° . On mixing they made the mercury in the thermometer sink rapidly, and as it

appeared to Mr. WALKER, below -40° . Its thread seemed to be divided below that point; but the froth occasioned by the ebullition of the materials prevented his making so accurate an observation as he could have wished.

The reason why this last mixture reduced the thermometer more than the third, though both were of the same materials, and the last at a lower temperature, Mr. WALKER imagines to have been partly because the fourth pan had not another immersed in it to give it heat, and partly because the materials were reduced to a finer powder.

I should imagine, that mercury reduced to its freezing point will freeze more quickly than water reduced to its freezing point, because it appears, from experiments on their capacity for heat, that the latter of these bodies has so much more latent heat in its liquid state; which greater quantity of latent heat must, as it becomes sensible, more retard the congelation.

I forbear to enumerate many variations of these experiments which Mr. WALKER has among his notes; but there is one mixture which, though its power is not equal to that which I have last described, may prove very serviceable in experiments of this nature, on account of its cheapness. It consists of oil of vitriol diluted with an equal weight of water: added to GLAUBER's salt, it produces about 46 degrees of cold. The addition of sal ammoniac renders it more intense by a few degrees. One remarkable circumstance occurred to Mr. WALKER, as he was endeavouring to ascertain the best strength of the vitriolic acid: he happened to be trying a mixture of two parts of oil of vitriol and one of water, when he observed, that, at the temperature of 35° , the mixture coagulated as if frozen, and the thermometer became stationary; but, on adding more GLAUBER's salt, it fell again, after some little time, but so great a

cold was not produced as when this circumstance did not occur, and when the acid was weaker. The same appearance of congelation took place with other proportions of acid and water, at other temperatures.

Mineral alkali, when it retained its water of crystallization, added to some of these mixtures heightened their effects. But when it had lost this water, it rather produced heat than cold; and the same thing is also true of GLAUBER's salt. This circumstance leads us, in some measure, to the theory of these phænomena. Water undoubtedly exists in a solid state in crystals; it must therefore, as in other cases, absorb a determinate quantity of fire, before it can return to its liquid state. On this depends the difference between GLAUBER's salt and fossil alkali in their different states of crystallization and efflorescence. The same circumstance too enables us to understand the great effect of GLAUBER's salt, which, as far as I recollect, has the greatest quantity of water of crystallization.

Those, therefore, who shall choose to pursue the path which Mr. WALKER has opened to them, would do well to try combinations of salts containing much water of crystallization; but they must take care lest the effect should be diminished or destroyed by the formation of compounds that fix a smaller quantity of fire. It is, however, but justice to Mr. WALKER to observe, that he has carried his experiments in this way very far, and with great ingenuity.

I have the honour to be, &c.

THOMAS BEDDOES.



XXVII. *An Account of a Doubler of Electricity, or a Machine by which the least conceivable Quantity of positive or negative Electricity may be continually doubled, till it becomes perceptible by common Electrometers, or visible in Sparks. By the Rev. Abraham Bennet, M. A.; communicated by the Rev. Richard Kaye, LL.D. F. R. S.*

Read May 10, 1787.

THE great importance of a machine for the purpose of detecting very minute quantities of electricity has occurred to many of the cultivators of this science; as by such an assistant not only many chemical combinations or solutions, but also many yet unexplained atmospheric phænomena, may become intelligible.

The labours of M. VOLTA have been very successful on this subject by the application of his condenser (as he terms it), which, by means of a thin-coated electric, is capable of receiving a greater quantity of the electrical fluid than a common insulated conductor, and rendering it perceptible by separating the positive and negative sides of the charged plate. On this ingenious contrivance Mr. CAVALLO made a very considerable improvement by transferring the received quantity of electricity from a larger to a smaller condenser, as explained in the Phil. Trans. Vol. LXXII.

Notwithstanding the very great sensibility of this apparatus, the electricity of the atmosphere is sometimes too weak to be discoverable by it: for instance, in some showers, when the negative state of the falling rain is nearly equal to the positive state of the air. Add to this the trouble of keeping an insulated and elevated conductor sufficiently dry, and the danger

ger of it in a thunder-storm. I therefore contrived the following doubler for the purpose of more easily making an electrico-meteorological diary, which I undertook at the request of my friend Dr. DARWIN, who hoped, that from thence some lights might be thrown on the causes of the sudden changes of aerial currents, a circumstance of so much importance to the early growth and maturity of vegetation.

I place upon my electrometer, described in a former Part of the Philosophical Transactions, a circular brass plate, three or four inches in diameter, polished and thinly varnished on the upper surface. On this I place another brass plate, of equal diameter, polished and varnished on both sides, with an insulating handle attached to one edge of it. A third plate is also provided, of equal diameter, polished and varnished on the under side, and with a perpendicular insulating handle from the center of the upper side, similar to those mentioned in the Appendix to my last Paper.

The method of collecting electricity from the atmosphere, and continually doubling it as much as required, is as follows. If the weather be dry, I carry into the open air a lighted torch, not liable to be easily blown out, or a small lantern with a lighted candle in it, to the bottom of which is fixed, by means of a socket, an insulating handle of glass covered with sealing-wax; in the other hand is carried a coated phial: then, elevating the flame a little higher than my head, I apply to it the knob of the phial, holding it in this situation about half a minute. Then returning into the house (where the above described doubler is kept dry, by being placed on a table not far from the fire), I apply the knob of the phial to the under side of the first plate, which lies immediately upon the electrometer, and at the same time touching the second plate with a finger of the other hand. Then laying aside the phial, I lift up the
the

the second plate by its insulating handle, and if the electricity be not now sensible by the electrometer, I place the third plate, by means of its insulating handle, upon the second plate, thus elevated: then touching the third plate, by stretching a finger over the juncture of its insulating handle, and again withdrawing the finger, I then again separate the third plate from the second. In this situation it will be apparent to electricians that two of the plates are of one kind of electricity, and nearly of equal quantity, and one only of the other. I then apply the third plate to touch the under surface of the first plate which remains on the electrometer, and at the same time covering the first plate with the second, I then touch the second plate by stretching a finger over the juncture of its insulating handle; and first taking away the third plate, and then withdrawing my finger from the second, and lifting it up from the first plate, the electricity becomes doubled. If by this first operation the quantity of electricity does not become sensible by the electrometer, I repeat the process to ten or twenty times, which, by doubling it every time, makes visible the smallest conceivable quantity of electricity, since, at the twentieth operation, it is augmented to above 500,000 times. And though in description the above process of doubling to twenty times may appear tedious, yet when the operator can perform it with sufficient readiness (which is soon acquired) it takes less time than 40 seconds. The collection of electricity from the air, and the touching and position of the plates, are represented in Tab. XI. figures 1. 2. 3. 4. 5. and 6.

If it be required to produce sparks, the plates are to be placed upon an insulating stand, without an electrometer, and the process repeated as above till the sparks appear.

The experiment which proves that the electricity is doubled by each operation is this. If the two slips of pendulous leaf gold

gold of the electrometer be made to diverge to a certain distance by the above process, that distance will be nearly doubled by repeating the operation. Another proof of this duplicate accumulation is, that, when the third plate is applied to the first, the divergency of the leaf gold is apparently undiminished, though in this situation their electricity is diffused over double the quantity of surface.

That flame will collect electricity better than points was mentioned in my former Paper, and is very evident if two phials of equal capacity are exposed to the air, the one furnished with a sharp point, and the other having its knob applied to an insulated flame, and their electricity afterwards examined by the doubler.

If the weather be rainy, an insulated umbrella may be carried in one hand, and the knob of the phial applied to the upper and insulated part of the handle; and if it rains so slowly as not sufficiently to communicate electricity to the umbrella, a torch is carried under the umbrella, and used as described above.

It is obvious that some caution is necessary in managing experiments of so much nicety, since, by the least friction of the hand on the varnished sides of the plates or insulating handles, or if the metallic side of one plate be accidentally rubbed against the varnished side of the other, some degree of electricity is produced, which, becoming sensible by the operation of doubling, may render the experiments equivocal.

To obviate these inconveniencies, I join a conducting handle, by means of an insulating nut, to each of the plates. This handle consists of turned unbaked mahogany, about three inches long, into one end of which is inserted a nut of baked wood, about half an inch long, covered with sealing-wax, upon the other end of which nut the brass socket of the plate is fixed; by this means it is not necessary to touch the sealing-

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wax of the insulating nut, but occasionally to stretch a finger over it to touch the plate, whilst the mahogany handle is held in the same hand.

Having found, by repeated experiments, that two clean metallic plates, or two equally varnished plates, rubbed together, produced no electricity, I varnished the second plate on both sides, but more thinly than when one side only was varnished, and in some experiments used thimbles on the ends of the touching fingers. In this way the inconveniencies of accidental friction were in some measure obviated, but much less than I first expected; for, notwithstanding the utmost care, electricity is produced without previous communication: therefore, in experiments requiring the electricity to be often doubled, its communication may yet be ascertained by applying it to the first and second plates alternately; so that positive electricity communicated to the first plate appears positive by the electrometer; but the same electricity, applied to the second plate whilst the first is touched, produces negative in the electrometer.

I beg leave to add, that this method of doubling either positive or negative electricity, as well as M. VOLTA's condenser, with Mr. CAVALLLO's improvement on it, as also the ingenious experiments of Father BECCARIA with double plates of glass, which he separated after charging, are all of them to be explained from the same principles with the Leyden bottle, of which they may be all said to be only different applications. I shall not therefore trouble the Society with any further theory on this subject, but proceed to lay before them the diary which I have hitherto kept, and during which time I have found no difficulty in collecting electricity from the atmosphere positive or negative, so as to become sufficiently sensible by the above described apparatus, though the hygrometer has sometimes shewn the greatest degree of moisture.

Diary

Electricity.

Number of times

lifted up.

stronger than before.

strong bottle was this day tried, which

lifted up.

lifted up; divergency of the leaf goes
weaker.

doubled.

lifted up.

hygrometer was higher than known

doubled.

Diary

The atmospherical electricity was sometimes so strong as to need no doubling, and mostly required only one application of the second plate, yet I frequently found it necessary to repeat the process from two or three to twenty times. Perhaps the exact comparative quantity of electricity residing in the atmosphere might be measured by the number of operations required to render it perceptible by the electrometer, all other circumstances being cautiously attended to.

If the electricity of the atmosphere should happen to be much weaker than I have yet found it, there remains not only the resource of doubling oftener, but the capacity of the instruments may be much increased; as, first, by using a larger flame; secondly, by elevating it higher; thirdly, by collecting the electricity with a very thin glass ball, silvered within, and coated on the outside in the common way, or gilt; fourthly, by grinding and polishing the plates of the doubler very exactly; fifthly, by making the experiments in an advantageous situation. In all these particulars my apparatus was defective, yet amply sufficient for the discovery of the atmospherical electricity.

After considering the successful effect of flame, in collecting atmospherical electricity, I placed an insulated lantern upon a pole about fifteen feet high, and suspending a gold thread from the lantern connected it with the electrometer, and was agreeably amused with seeing the pendulous gold leaf open and shut with every passing cloud.

On the 27th of February, 1787, when there was a considerable mist whilst the lantern was thus elevated, the leaf gold frequently struck the sides of the electrometer; and, in

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X x

about

about an hour, some drops of rain beginning to fall, the appearance of electricity with this apparatus entirely ceased, though I elevated the lantern several times in the course of the day.



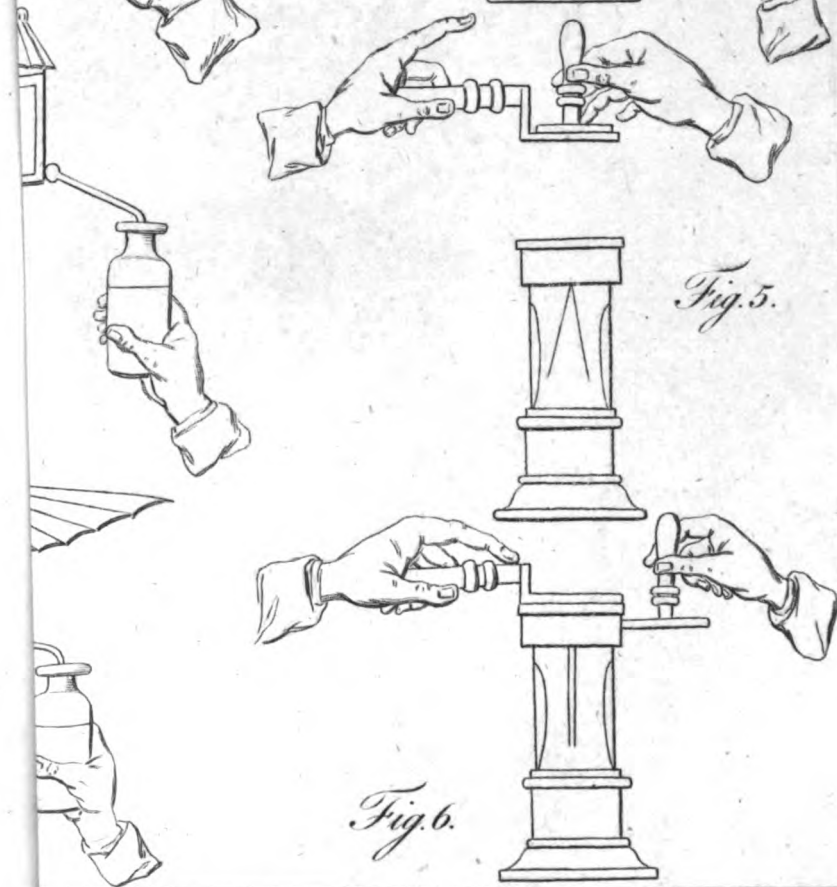
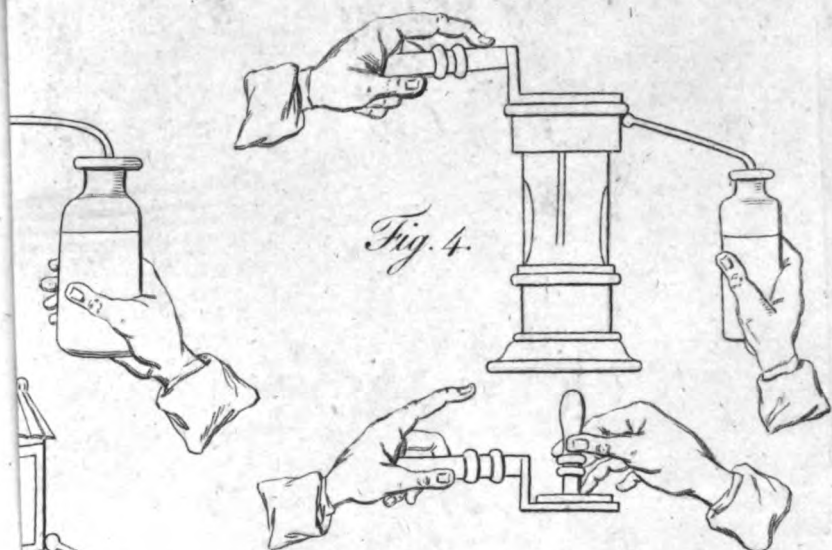


Fig. 6.

Repertoire de.

XXVIII. *Some Particulars relative to the Production of Borax.*
In a Letter from William Blane, Esq. to Gilbert Blane, M.D.
F. R. S.

Read May 17, 1787.

MY journey to the northern mountains in January last, in attendance upon the Vizier, gave me an opportunity of satisfying, in some degree, my curiosity on the subject you are so desirous of being informed of, the production and manufacture of Borax. The place which his Excellency visited is called Betowle, and is a small principality in the first of the northern mountains, where they rise from the plains of Hindostan, and is distant from Lucknow about 200 miles N.E. The town is a principal mart, where the commodities of the mountains are exchanged for those of the plain. The Raja, or Prince of the country, holds his possessions in the hills as an independent sovereign; but for those on the plain he owes fealty, and pays tribute to the Vizier. He therefore embraced this opportunity of paying homage in person to his Lord. During his stay at court, I had an opportunity of making the enquiries I wished from his people, and particularly from his Dewan or Minister, who had with him some of the inhabitants of the place where the borax is made.

This saline substance, called in the language of this country *Swagah*, is brought into Hindostan from the mountains of Tibbet. The place where it is produced is in the kingdom of

X x 2

Jumlate,

Jumlate, distant from Betowle about thirty days journey north. Jumlate is the largest of the kingdoms in that part of the Tibbet mountains, and is considered as holding a superiority over all the rest.

The place where the borax is produced is described to be in a small valley, surrounded with snowy mountains, in which is a lake, about six miles in circumference, the water of which is constantly hot, so much so that the hand cannot be held in it for any time. The ground round the banks of the lake is perfectly barren, not producing even a blade of grass; and the earth is full of a saline matter in such plenty that, after falls of rain or snow, it concretes in white flakes upon the surface, like the *natron* in Hindostan. Upon the banks of this lake, in the winter season, when the falls of snow begin, the earth is formed into small reservoirs, by raising it into banks about six inches high; when these are filled with snow, the hot water from the lake is thrown upon it, which, together with the water from the melted snow, remains in the reservoir, to be partly absorbed by the earth, and partly evaporated by the sun; after which, there remains at the bottom a cake, of sometimes half an inch thick, of crude borax, which is taken up and reserved for use. It can only be made in the winter season, because the falls of snow are indispensably requisite, and also because the saline appearances upon the earth are strongest at that season. When once it has been made upon any spot, in the manner above described, it cannot be made again upon the same place, till the snow shall have fallen upon it and dissolved three or four times; after which the saline efflorescence re-appears, and it is again fit for the operation.

The borax, in the state above described, is transported from hill to hill upon goats, and passes through many different hands before

before it reaches the plains, which increases the difficulty of obtaining authentic information regarding the original manufacture. When brought down from the hills, it is refined from the earth and gross impurities by boiling and crystallisation. I could obtain no answers to any questions regarding the quality of the water, and the mineral productions of the soil. All they could say of the former was, that it was very hot, very foul, and as it were greasy; that it boils up in many places, and has a very offensive smell: and the latter remarkable only for the saline appearances above described. That country, however, in general, produces considerable quantities of iron, copper, and sulphur. After being purified it sells in the market here for about 15 rupees *per* maund; and I am assured, by many of the natives, that all the borax in India comes only from the place above mentioned.

I am afraid you will think this at best but a very unsatisfactory and unphilosophical account of the matter; but what can be done, where the only mode of information is through some of the wild and unsettled mountaineers? for the place is inaccessible even to the inhabitants of Hindostan, and has never been visited by any of them, except a few wandering Faquires, who have been sometimes led that way, either to do penance, or to visit some of the temples in the mountains. The cold in winter is described to be so intense that every thing is frozen up, and that life can only be preserved by loads of blankets and skins. In the summer again, the reflection from the sides of the mountains, which are steep and close to each other (there being little or no plain ground betwixt them), renders the heats insufferable.

I have not loaded this account with any reflections or conjectures of my own. I have simply given you the narrative of those from whom I had my information; and having put into
your

your possession all the *data* I have been able to collect upon the subject, you may make what use of them you please.

I shall conclude with a few observations regarding the credibility of the relation: and, first, that it is really brought from the Tibbet mountains is certain, as I have myself often had occasion to see large quantities of it brought down, and have purchased from the Tartar mountaineers, who brought it to market; secondly, I have never heard of its being either produced or brought into this country from any other quarter; and, thirdly, if it was made on the Coromandel coast, as some books mention, I think there can be little doubt, but that the whole process would have been fully enquired into, and given to the public long before this time.

Lucknow, August 28, 1786.



XXIX. *A Letter from the Father Prefect of the Mission in Tibet, F. Joseph da Rovato, containing some Observations relative to Borax. Communicated by Sir Joseph Banks, Bart. P. R. S.*

Read May 17, 1787.

Alli illustrissimi Signori Membri dell' Accademia Reale delle Scienze ed Arti di Londra.

IL Padre prefetto della Missione del Tibet ha il piacere di manifestare alle SS. VV. illustrissime, come essendo in Patna stato ricercato più volte dal Sig. VOGLES, Alemano, bravo naturalista, di prendere notizia dei luoghi, e del come si faceva il Borace, che si ha dal regno del gran Tibet; giacchè come egli diceva nessun alto poteva avere comunicazione da quelle parti quasi impenetrabili; quantunque la nostra missione abbia abbandonato da molti anni detto regno, nonostante avendo amicizia con il *Bahadur Sah*, fratello del re di Nepal (il regno del quale si estende a tramontana fino a Kutti, frontiera del Tibet) li scrisse, e lo pregò a prendere la notizia dei luoghi, e del come si faceva il borace, ed a comunicargliela. Il Bahadur Sah avuta questa istanza, per meglio favorire il Padre prefetto, avendo al suo servizio un uomo dello stesso paese dove si forma il borace,

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si degnò di mandarglielo fino a Patna, per intendere dallo stesso uomo tutto ciò che desiderava intorno al borace.

Questo uomo, parte in lingua Nepalese, parte in lingua Indostana, ambedue intese dal Padre prefetto, si spiegò come segue. Nella provincia, ossia territorio di Marmè, distante 28 giornate di cammino a tramontana del Nepal, e 25 dette al ponente di Lassa capitale del Tibet, vi è una valle larga 8 miglia in circa. In un distretto di questa valle vi sono due terre, o Castelli, uno chiamato *Scierugh*, e l'alto *Kanglè*; e li uomini di questi due luoghi s'impiegano a cavare il borace, e lo vendono nel Tibet, e nel Nepal, non avendo altro mezzo per vivere, stante che in detta valle il terreno è sterile, e non produce che alcuni giunchi. Vicino ai sudetti due Castelli vi è una Vasca di acqua, che non è molto grande, e varie altre più piccole (dove il terreno è concavo) nelle quali resta l'acqua quando piove. In queste Vascche dunque, dopo qualche tempo che vi è restata l'acqua, si forma da per se il borace; e li uomini entrando nell'acqua, dove sentono con li piedi come un lastrico, ivi il borace è formato, e lo cavano; con questa distinzione, che dove è poca acqua, il borace è più sottile, e lo sentono subito; e dove l'acqua è molto lo trovano più grosso, ed a questo vi resta sopra un dito, o due, di fango molle, che sarà una deposizione della stessa dopo ch'è stata intorbidata dalla pioggia, o dal vento. Così si trova il borace prodotto dalla natura senza uso alcuno nè di bollitura, nè di lambicco; e l'acqua nella quale si produce il borace è così cattiva, che se alcuno ne beve un poco, li gonfia il ventre e lo fa morire. La terra, in cui si produce il borace, è di colore bianchiccio; e 4 miglia in distanza dove si produce il borace, nella stessa valle vi sono le miniere del sale, che si cava in grande abbondanza per uso di tutti quei popoli dentro le monti, che sono

sono così lontani dal mare. Li nativi non pagano nulla a cavare il borace, non avendo altro mezzo per vivere a causa della sterilità del terreno; ma se li stranieri vogliono cavare il borace è necessario che paghino un tanto al Capo del luogo secondo la convenzione che fanno; e nella valle di Marmè pagano un Lamà chiamato *Pema tupkan*, a cui appartengono le miniere del borace.

Altre 10 giornate di cammino più a tramontana della valle di Marmè vi è un'altra valle chiamata *Taprè*, nella quale pure si cava il borace. Vi è ancora un altro luogo, in cui si cava il borace, chiamato *Ciogà*, ma di questo ultimo non ho marcata la situazione. Il borace in lingua Indostana e Nepalese si chiama *Soagd*; ma se non è purificato svanisce facilmente; e per conservarlo qualche tempo finchè lo vendono, lo mischiano con la terra unta di buttiro.

Nel territorio pure di *Mungdan* 16 giornate più a tramontana del Nepal vi sono abbondanti miniere dell'arsenico; ed in molti altri luoghi le miniere del solfo, come pure le miniere dell'oro, e dell'argento, che si cava assai più puro di quello che si cava dalle miniere del Pegù.

Questo è quanto si è ricavato dal uomo mandato dal Bahadur Sah, fratello del re di Nepal. E se gli Signori dell'Accademia Reale desiderano di vedere un poco di quella terra, nella quale si produce il borace, ora che tutto il paese di Nepal è governato dallo stesso Bahadur Sah; il Padre prefetto, che lo ha praticato per alcuni anni dentro lo stesso Nepal, e due volte ancora a Patna (ove era venuto) spera che non gli negarà il favore di mandare qualche suo fidato uomo a prendere di detta terra, e inviargliela a Patna; dal qual luogo potrà il Padre prefetto, mediante qualche Sig. Inglese suo amico, fargliela con tutta facilità pervenire a Londra.

Questo è quanto il Padre prefetto prende la libertà di scrivere alli illustrissimi Signori dell'Accademia, alli pure si offerisce di

buon cuore con li altri suoi Religiosi Cappuccini Italiani, se potrà servirli per qualche altra notizia di questo genere, per mostrare la sua gratitudine alla nazione Inglese, dalla quale ha ricevuti, e riceve tanti benefizi; ed ha l'onore di rafferinarsi con tutto il rispetto, &c.

F. GIUSEPPE DA ROVATO.

Patna, li 10. 7bre;
1786.



XXX. Sur les Gas Hepatiques: par Monsieur Hassenfratz.
Communicated by Sir Joseph Banks, Bart. P. R. S.

Read May 17, 1787.

EN 1785, après avoir fait quelques expériences sur les foies de soufre, je résolus de chercher quel étoit la nature de l'air inflammable hépatique que l'on en faisoit dégager en les décomposant par l'acide nitreux. Le soufre que l'on voit se précipiter après chaque combustion de gas inflammable hépatique me portait à croire que cette substance pouvait bien être une de ses parties constituantes, mais elle ne m'en apprenait pas davantage, et c'étoit dans ce tems à-peu-près la seule expérience que l'analyse pouvait me fournir. Je résolus donc d'employer la synthèse, et ce qui me conduisit à cette résolution fut la citation d'une expérience que M. MONGES avait fait à Meziers; il avait fait passer de l'air fixe à travers du soufre en fusion, et il avait eu pour résultat de l'air fixe avec une odeur sulfureuse, et qui tenait réellement du soufre en dissolution. Je me déterminai donc à répéter son expérience, et à tenter celle que je vais citer.

Je fis d'abord passer de l'air fixe à travers du soufre en fusion, et j'eus pour résultat un air fixe sulfureux qui faisoit précipiter la chaux vive de sa dissolution dans l'eau distillée, en s'unissant à cette base, formant par cet union, de la terre calcaire, et laissant dégager le soufre qui fumageait le liquide, ou se précipitait au fond. Je fis la même expérience avec le gas nitreux, et j'eus pour

Y y 2

résultat

résumé un gas nitreux sulfureux, qui se combinait encore avec l'air vital pour former de l'acide nitreux, et abandonnait le soufre dans cette combinaison.

La mofète atmosphérique m'a produit, après son passage, de la mofète sulfureuse avec odeur de soufre, qu'elle laissait précipiter après avoir séjourné quelque tems sur l'eau.

L'air vital a produit dans une semblable expérience un air vital sulfureux, mélangé d'acide sulfureux volatil, que l'eau absorbait : le gas nitreux, en s'unissant avec rutilance à l'air vital sulfureux, en faisait dégager le soufre, de même que sa détonnation avec le gas inflammable.

L'air atmosphérique m'a donné un résultat à-peu-près semblable à l'air vital, seulement, ainsi qu'il est aisé de le déduire des expériences précédentes, l'air que j'obtins était un mélange d'air vital et de mofète sulfureuse. Le gas nitreux par son union avec l'air vital et le gas inflammable dans sa détonnation faisait précipiter le soufre de son mélange.

Enfin le gas inflammable, passé à travers du soufre en fusion, m'a donné un gas inflammable sulfureux, tout-à-fait semblable au gas hépatique que l'on obtient en versant de l'acide nitreux sur du foie de soufre : d'où il paroît suivre que ce gas inflammable que l'on retire des foies de soufre en y versant de l'acide nitreux, et auquel on a donné le nom de gas hépatique, n'est autre chose qu'un gas inflammable sulfureux, que l'on peut former synthétiquement, ainsi que toutes les autres espèces de gas sulphureux.



XXXI. Botanical Description of the Benjamin Tree of Sumatra.

By Jonas Dryander, M. A. Libr. R. S. and Member of the Royal Academy of Sciences at Stockholm; communicated by Sir Joseph Banks, Bart. P. R. S.

Read May 17, 1787.

THOUGH GARCIAS AB HORTO, GRIM, and SYLVIVS, were acquainted with the real tree from which Benjamin or Benzoin is collected, their descriptions of it are so imperfect and insufficient for its botanical determination, that succeeding botanists have fallen into many errors concerning it; and it is remarkable, that although this drug was always imported from the East-Indies, most of the later writers on the Materia Medica have conceived it to be collected from a species of *Laurus*, native of Virginia, to which, from this erroneous supposition they have given the trivial name of Benzoin. This mistake seems to have originated with Mr. RAY, who in his *Historia Plantarum*, Vol. II. p. 1845. at the end of his account of the *Arbor Benivifera* of GARCIAS, says: “Ad nos scripsit D. TANCREDUS ROBINSON *Arboresc. resiniferam odoratam foliis citrinis prædictæ haud absimilem transmissam fuisse e Virginia a D. BANISTER, ad illustrissimum Præfulem D. HENR. COMPTON, in cujus instructissimo horto culta est.*—*Arbor ista Virginiana Citrii, vel Limonii foliis Benzoinum fundens, in horto reverendissimi Episcopi culta.*”

This error was detected by LINNÆUS, but another was substituted by him in its place; for in his *Mantissa Plantarum*.

Alteram,

Altera, he tells us, that Benjamin is furnished by a shrub described there under the name of Croton Benzoe, and afterwards in the Supplementum Plantarum, describes again the same plant, under the name of Terminalia Benzoin. M. JACQUIN, who had been informed that this shrub was called by the French *Bienjoint*, supposes, with reason, that the similar sound of that word with *Benjoin*, the French name for Benjamin, may have occasioned this mistake *.

Since that period Dr. HOUTTUYN has described the Benjamin Tree of Sumatra; but for want of good specimens has been so unfortunate as to mistake the genus to which it belongs. It is hoped, therefore, that the following description and annexed figure (see Tab. XII.) may not be unworthy a place in the Philosophical Transactions; they are made from dried specimens procured from Sumatra by Mr. MARSDEN, F. R. S. at the request of Sir JOSEPH BANKS, Bart. P. R. S. and clearly prove that this tree agrees in the parts of fructification with the *Styrax* of LINNÆUS.

STYRAX *Benzoin*, foliis oblongis acuminatis subtus tomentosis, racemis compositis longitudine foliorum.

Benjui. *Garcias ab Horto in Clusii Exoticis*, p. 155.

Arbor Benzoini. *Grim in Ephemer. Acad. Nat. Curios. Dec. 2. Ann. I. pag. 370. fig. 31. Sylvius in Valentini Historia Simplicium*, pag. 487.

Benzuin. *Radermacher in Act. Societ. Bataviæ, vol. III. pag. 44.*

Benjamin or Benzoin. *Marsden's Hist. of Sumatra*, pag. 123.

Laurus Benzoin. Houttuyn in Act. Harlem. vol. XXI. pag. 265. tab. 7.

Habitat in Sumatra. h.

* Hort. Vindob. vol. III. p. 51.

DESCRIPTION.

Rami teretes, tomentosi.

Folia alterna, petiolata, oblonga, integerrima, acuminata, venosa, supra glabra, subtus tomentosa, palmaria. *Petiolis* teretes, striati, canaliculati, tomentosi, brevissimi.

Racemi axillares, compositi, longitudine fere foliorum. *Pedunculi communes* tomentosi; *partiales* alterni, patentes, tomentosi. *Pedicelli* brevissimi. *Flores* secundi.

Calyx campanulatus, obsolete quinquedentatus, extus tomentosus, linea longior.

Petala quinque, (basi forte connata) linearia, obtusa, extus tomento tenuissimo cinerea, calyce quadruplo longiora.

Filamenta decem, receptaculo inserta, petalis paulo breviora, inferne connata in cylindrum longitudine calycis, superne infra antheras ciliata. *Antheræ* lineares, filamentis longitudinaliter adnatæ, iisque dimidio breviores.

Germen superum, ovatum, tomentosum. *Stylus* filiformis, staminibus longior. *Stigma* simplex.



XXXII. *An Account of an Experiment on Heat.* By George Fordyce, M. D. F. R. S. *In a Letter to Sir Joseph Banks, Bart. P. R. S.*

Read May 24, 1787.

S I R,

H EAT changes the qualities and appearances of matter in various ways. It is also a powerful agent in many of the operations which mankind employ to fit matter for their use. Although the ancients performed many of these operations with a considerable degree of accuracy, yet there are many which they were totally unacquainted with, and others they brought to little perfection. One principal cause was their having no means of measuring heat accurately. VAN HELMONT was the first who found the mode of measuring heat by expansion. His measure was an air thermometer, which is described in his Dissertation, named "*Aer*", cap. 12. Since his time, various improvements have been made on thermometers; many are still wanted. This instrument is, however, the foundation of modern discoveries on this subject. The ancients were acquainted with the manner of heating bodies by communication, by friction, by burning fuel, by the sun, by fermentation, and the taking place of chemical combinations in other cases. BOYLE found, that melting a solid body produced cold (*Experimental History of Cold*, title I. chap. 18.):

Dr. CULLEN, that cold was also produced by converting bodies into vapour. It has been since that time found, that the opposite condensations, *viz.* of vapours into fluids, or fluids into solids, generate heat. CRAMER was the first who took notice of the different conducting powers of different bodies, in his "*Ars Docimastica*," P. I. § 274. Scholium.

The power of animal bodies, of resisting the cold of the medium they are in, has been long known. Mr. ELLIS took notice of their being also able to resist the heat. Dr. CULLEN ascribed this power to a peculiar quality in animals different from the powers of inanimate matter. You, Sir, saw a confirmation of this power being very great when we kept a dog of no large size (he might weigh, as far as I can recollect, about twenty-five pounds, not more) in air heated to 160 degrees of FAHRENHEIT's thermometer for half an hour. We took him out with only the addition of a few degrees of heat; not from any uneasiness of the animal, but from being satisfied with the experiment. This power has been shewn by Mr. HUNTER to extend to vegetables. The degree of heat one body is capable of impregnating another with, was hardly touched upon by any author before Dr. CRAWFORD, who has done a great deal in this branch, and is still pursuing it.

The subject of the present enquiry is different from all these. The proposition is, supposing we can make an application to a cold body, so as to produce heat in it, and this application be made with the same force to the same body, whether by this means an equal quantity of heat will always be produced in an equal quantity of matter? That is, for instance, whether an equal quantity of the rays of the sun being thrown on an equal surface of the same matter, so that they shall be equally lost, bent, or reflected, an equal mass of matter below shall be

equally heated according to its capacity ; whether equal vibrations excited shall always produce the same quantity of heat ; whether a chemical attraction taking place between an equal quantity of two substances shall always produce an equal quantity of heat ?

The importance of this enquiry is sufficiently evident, since if the same quantity of fuel being burnt the same quantity of heat be always produced, our whole attention will be to take care that no part of the heat shall be lost ; but if burning the fuel under one set of circumstances will actually produce a greater quantity of heat than burning it in other circumstances ; or if burning it, will produce a great heat in one place, which cannot be carried to another place, but will be again annihilated, a very different attention must be paid. I was first led into this train of thinking by observing reverberatory furnaces. Formerly I had no doubt but that it was obvious, that the same quantity of fuel burnt would produce the same quantity of heat ; but having occasion to try some experiments in reverberatory furnaces, where great heat and cleanness were required, I tried to heat the furnace with charcoal and coak, or pit-coal charred, that is, burnt till no smoke arises, but could never produce the heat required, although I could do it easily with coal. I insulated my furnace, so that after twenty-four hours strongest fire, it did not feel in the least warm on the outside. I heightened the chimney ; but all to no effect : in the fire-place the heat was sufficient to melt malleable iron, but in the laboratory, in the horizontal part of the chimney, the heat was trifling. Since that time I have made various experiments to ascertain the proposition laid down. The following one, which has been varied and repeated with the same result, may, perhaps, draw the attention of chemists to this point.

I formed a cylinder of thin pasteboard, six inches diameter and sixteen inches long. The inside I lined with rabbit skin, laying the fur smooth; a thin ring of pasteboard was placed in the middle. One end was closed with a bottom of the same pasteboard; the other was open. This cylinder was placed in the center of another wider cylinder, also of pasteboard, which had likewise a bottom of pasteboard. It was so placed, as that the outer cylinder was distant from the inner one inch and a half; at the bottom and sides the space between was filled with Eider down, suffered to rise to as great a bulk as it would from its own elasticity. The two cylinders were even at top, and the space between them shut by a cover of pasteboard. In the side of the machine, a little below the middle of the inner cylinder, a pasteboard tube was made to pass through the outer and open into the inner, half an inch wide, for the insertion of a thermometer. A similar tube was placed a little from the middle, towards the other end of the smallest cylinder. A circular plate, of pasteboard, six inches diameter, and about one-eighth thick, weighing 1 oz. 102 grs. was pushed down the inner cylinder, until it was stopped by the ring. A circle of flint glass, ground flat and parallel on both sides, was fixed over the mouth of the inner cylinder so as not to obstruct any part of it.

A similar apparatus, as exactly as possible, was formed, excepting that the circular plate in the middle of the inner cylinder was iron, of the same dimensions with the pasteboard one, and weighing 12 oz. 62 grs. These apparatus's were set in a warm exposure for several months, to dry.

The circular plates, which were destined to receive the direct rays of the sun, were placed as nearly perpendicular to the inner cylinder as possible. They were both covered with a

black paint, sufficient to prevent the rays of the sun from penetrating either to the iron or pasteboard.

On the 28th day of July, 1786, the sun shining on a room facing about S.W. the air not cloudy, but not very bright; the air in the room 71° ; at a quarter after twelve, thermometers being passed through the tubes below the plates of iron and pasteboard, after standing a quarter of an hour, shewed the heat 67° in both apparatus's. Both were now exposed to the sun, so that the rays fell perpendicular on the paint covering the plates, in equal quantity on each as nearly as possible. If there was any difference, rather more were thrown on the pasteboard diaphragm. In five minutes the thermometer below the pasteboard diaphragm shewed 72 degrees; the thermometer under the iron had hardly risen half a degree.

Progress of the rising of the thermometers.

Under pasteboard diaphragm.	Under iron diaphragm.
72°	67½
75	70
80	76 +
85	83
90	88 +
95	94
100	100 *
105	107
110	115
After 20 minutes,	
110	121

* At this time thermometers were put through tubes into the chambers of the apparatus, between the glasses and diaphragms. The apparatus with the iron diaphragm raised this thermometer to 121° ; that with the pasteboard to 120° .

The

The apparatus with the pasteboard diaphragm was exposed still to the sun; that with the iron was removed, and suffered to cool till its thermometer shewed 107° ; it was then exposed again to the sun till it had acquired the heat of 110° , to which degree the apparatus with the pasteboard hardly reached. The windows were now shut. The heat of the room had arisen to 80° . Both the apparatus's were placed on a table; the doors were shut, so that there was no current of air.

Pasteboard apparatus, after 30 minutes.	Iron apparatus.
96	104
After 75 minutes, or 1 h. 45' from the beginning.	
83	89
After 2 h. or 3 h. 45' from removal from the sun;	
78	80
room 75	

A similar result arose when there were no glasses to exclude the external air. Likewise when the diaphragms were changed from one apparatus to the other.

If any one wishes to repeat these experiments, he must take care that the size of both apparatus's be the same; the weight the same, excepting the difference of the iron and pasteboard; that they be equally stuffed, and perfectly dry: for if there be the least moisture, the evaporation will not only make a fallacy in the experiment, but it will soon obscure the glasses, so as to prevent the rays of the sun from passing through them.

The first thing to be noted in this experiment is, that the rays of the sun acted on the same black paint only: for it was so thick, that the rays could not penetrate to the iron or pasteboard below. The colour was the same, and there was the same gloss; if any thing, that on the iron, in the experiment related, was rather more glossy, in order that it might not be favoured,

favoured, as in former experiments the results had been in favour of the iron apparatus acquiring the greatest heat. Every thing, therefore, was the same, except that the iron and pasteboard were of different weights, of different capacities of heat, and of different degrees of readiness to acquire heat, and communicate it.

It is evident, that a greater quantity of heat was actually produced in the apparatus with the iron diaphragm: for although in the first two or three minutes the pasteboard became hotter than the iron, yet as soon as the iron began to be sensibly heated, it became hot faster than the pasteboard, and actually became hotter, and even continued to do so, when the pasteboard no longer could produce more heat than was dissipated from the surface of the apparatus into the air. When they were set in an air equally cold the apparatus with the iron diaphragm was longer in cooling, although they were both of the same degree of heat when set by.

This greater quantity of heat I ascribe to the iron's taking the heat from the black paint faster than the pasteboard, as being a better conductor. Just as if a plate of glass was placed on a plate of steel, and another, perfectly similar, was placed on a plate of clay, and both were placed equally among equal vibrating bodies. In this case it is clear, that much greater vibration would take place if the same means of exciting it were applied to that plate of glass attached to the plate of steel than if they were applied to that attached to the clay. I do not mean to say, that heat is vibration; but merely to illustrate my idea of heat's being only a quality, and not a substance. I am led to this not only by this experiment now related, but by various other considerations, which I shall not now insist upon, as they are not sufficiently finished to be laid before this Society.

I shall

I shall only add that, among other things which may be illustrated by it, one is, that all the planets may possibly be of the same heat ; since, if the matter of which Mercury consists was averse to the generation of heat in proportion to the greater number of the rays of the sun it receives more than the Georgium Sidus, they would be both of the same heat, notwithstanding their different distances from the sun.

I have already said, that I was led to an enquiry into the subject by the effect it has on chemical operations.

I remain, &c.

G. FORDYCE.



XXXIII. *Account of an Observation of the Right Ascension and Declination of Mercury out of the Meridian, near his greatest Elongation, Sept. 1786, made by Mr. John Sineaton, F. R. S. with an Equatorial Micrometer, of his own Invention and Workmanship; accompanied with an Investigation of a Method of allowing for Refraction in such Kind of Observations; communicated to the Rev. Nevil Maskelyne, D. D. F. R. S. and Astronomer Royal, and by him to the Royal Society.*

Read June 7, 1787.

M. DE LA LANDE having announced to some of my astronomical friends the utility of accurate observations of Mercury, at his two elongations the last year, in August and September; I tried to get observations of that planet in crossing the meridian, for some days before and after the greatest elongation in August; and though the state of the atmosphere about that time was not very favourable to the purpose, yet there was one day that I thought unexceptionable, but could not perceive the least appearance of Mercury; at which I was the rather surprised, as I had formerly seen that planet in the like situation, with the same instrument, with perfect perspicuity*: and as I did not hear of any one else having succeeded in

* The instrument mentioned is a transit, made by myself in the year 1768; at which time achromatic object-glasses not having been, so far as I knew, applied to astronomical instruments by others, but having found the good effects thereof for

in this observation, I thought it might be very possible for the same disappointment again to happen, with respect to the approaching elongation in September. I judged, therefore, that it might be of some utility to astronomy, if, by *any means*, a good observation of Mercury could be got; and also, that it would be a proper subject whereon to make trial of an instrument for such purposes, the idea of which I had conceived, and begun to construct, above forty years before; but which, from various avocations, I did not perfect to my satisfaction till the year 1770*: since which time it has lain by, in hopes that something might happen, by which a full and effectual trial might be made thereof.

This instrument was originally intended as an improvement of the common wire micrometer, for the purposes of taking differences of right ascension and declination, in a more commodious and effectual manner than could be done in the method then practised of using that instrument †; and at the same

for other purposes, I resolved to use a double achromatic object-glass, made by Mr. DOLLOND; which being of equal aperture with the simple object-glass, then in the transit of the Royal Observatory, this telescope I therefore esteemed to be of nearly equal validity, as to quantity of light, with that at Greenwich, but reduced to the more commodious length of three feet six inches.

* Some observations made therewith, after it was completed, I transmitted to my friend Mr. AUBERT; the consistency of which induced him to procure a similar instrument to be made for cometary and such kind of observations as cannot be commodiously made in the meridian. Of this instrument the Rev. Mr. WOELLASTON has made honourable mention in *Phil. Trans.* Vol. LXXV. for 1785, p. 348.

† The common wire micrometer, as used by Dr. BRADLEY, and described from a paper of the Doctor's hand-writing, is given by Dr. MASKELYNE in the *Philosophical Transactions*, Vol. LXII. for the year 1772; and in addition to which I must beg leave to observe, that the telescopes then in use for the

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same time more effectually to answer the purpose of GRAHAM'S *astronomical sector*, which was contrived by him (as Dr. SMITH informs us) to supply the deficiencies of the micrometer then in use*.

The most necessary and fundamental improvements seemed to be; first, that of rendering the micrometer telescope manageable upon an equatorial motion; and, secondly, the contrivance of a stand of such solidity and steadfastness that the telescope might preserve the position in which it was placed, for a length of time: for it occurred to me, that if the telescope could be maintained at rest; or in a degree of stability superior to that of the astronomical sector; then the necessity of taking in a greater compass in declination than could be commodiously given to the field of a telescope would be the less necessary: for, instead of confining the object to a comparison with a star not differing more than a few minutes of time, or at most a quarter of an hour in right ascension, these comparisons could be extended to an hour or two, or even on occasion to three or four hours; there being scarcely any part of the heavens so devoid of stars, of a suitable magnitude for these observations, but that a proper one may be found within that compass in right ascension, provided there is allowed

micrometer were from ten to fifteen feet long, made with wooden tubes, supported at each end upon two wooden supports, by which the telescope could be managed in altitude and azimuth; but, not to follow a celestial object in its proper motion on one center: which apparatus, I believe, is still remaining in the Royal Observatory.

* This instrument is described in SMITH'S *Optics*, Vol. II. p. 350; and the original one, made by Mr. GRAHAM, was, at his death, placed in the Royal Observatory, and is mounted upon a three-legged stand of wood.

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the difference of a degree, either north or south of the object, in declination *.

Considering, however, that the approaching elongation would be in the morning; and that the best chance of seeing Mercury with this instrument would be some time in the twilight, between Mercury's rising and the rising of the Sun; yet, on supposition of catching the planet in his passage over the wires, there would be no chance of seeing any star pass over the field, wherewith to compare him, till the following evening, which being at least fourteen hours, the certain position of the telescope for so great a length of time was *almost* more than I could reasonably hope for. To judge how far I might form an expectation, by way of a previous trial, I compared Saturn with γ Capricorni \dagger , and found the return

* The field of the telescope of Mr. AUBERT's instrument is two degrees; but that of the original, wherewith this observation was made, is $1^{\circ} 17'$: to gain which, the eye-glass being immoveable (and *achromatic* to prevent the indistinctness that would otherwise have taken place near the border) the magnifying power was obliged to be considerably reduced, in respect of what has been usual for micrometers, that is, so as not to exceed 20 times: in consequence, there is, therefore, no need for so long a telescope, this being but $34\frac{1}{2}$ inches focal length of object-glass; but being a double achromatic, made by the late Mr. JOHN DOLLOND, it is capable of as great an aperture as could be given to the simple object-glasses of twelve or fifteen feet telescopes, that were then generally given to micrometers; but the pencil of light being greatest in this, is attended with this advantage, that the small stars can be seen very distinct and in great abundance, like the modern night-glasses: and there is in reality no need of great magnifying powers for the present purpose; for the place of the wire being viewed by an eye-glass, of about $1\frac{1}{2}$ inches focus, its place may be distinguished to less than a 2300th part of an inch, which, on the radius of $34\frac{1}{2}$ inches, is scarce $2''\frac{1}{2}$ of a degree; and which, as I apprehend, is nearer the truth than can reasonably be expected from instruments out of the meridian.

\dagger According to this observation \dagger preceded γ by the 2d of Sept. at 9 h. 15' P.M. mean time by $30' 9''.7$ MT, and with greater declination south than γ by $41' 23''$.

of the star to the same place two evenings afterwards, both in right ascension and declination, was so near, that I concluded I might very well expect a good observation of Mercury, in case I could get a sight of him, though the stars wherewith he was to be compared lay at the distance of the following evening at the soonest.

The micrometer is furnished with five horary wires, denominated in their order *a*, A, B, C, D (B being the middle horary wire), and the two declination wires are denominated *A* and *B*, each moveable by a separate and independent micrometer-screw, from the outside of the field to the center, and a little beyond it; so that each wire can be moved into the place of the other when at or near the center*.

The morning of the 23d of September, about a quarter past five o'clock, the air being clear and perfectly serene, it being then about an hour after Mercury's rising, and near three-quarters of an hour before the rising of the sun, I very readily found Mercury with the telescope, and when found could easily see him with an opera glass; and Mercury being then in a state of very little alteration of declination, I adjusted one of the declination wires to his apparent run, by making him traverse the *whole* field. The observations were then taken as

* In Dr. BRADLEY's Paper it is said, that before the *late* alterations, both the declination wires were made moveable; and that it was an improvement to make one of them fixed, and one only moveable. But however they might be immediately preceding the Doctor's time, I believe, the original micrometers by Mr. TOWNLEY were with one fixed and one moveable declination wire, as I have seen one in this form among the remaining apparatus of Mr. ABRAHAM SHARPE. In an instrument, however, fitted up for the purposes of the *equatorial micrometer*, I believe, it will be found most convenient to have both those wires moveable; as by this means they not only are enabled to slide into *each other's place*, but every part of the frame of the instrument remains fixed during the whole of the observation, the two slides carrying these two wires excepted.

in the first table; and in the evening I was lucky enough to get those of λ Ceti and σ Tauri, intending to repeat the whole the next morning and evening. The next morning proved cloudy, and so continued, that I saw the planet no more; but in the evening of the 26th, I found the stars come again so near the same declination, that I was encouraged to continue the observation to see what change would happen. It then came on bad rainy weather till the 30th, when I again repeated the observation, and found the stars to come so near in declination that I was fully satisfied of the stability of the instrument, so far at least as could regard twenty-four hours: but as I was then appointed to go a journey, and could have no other use for it, I locked the door of the Observatory, leaving the instrument in its position, that I might see what change would happen by the time of my return; and was quite astonished to find, on the 13th of October, that it had remained in a manner unmoved; for it had suffered no more apparent alteration than what might occur by the errors of observing, and alterations of the clocks and transit.

It must, however, be remarked, that, besides that in the construction of the instrument every thing was contrived that appeared likely to give it firmness, it was rested upon the *frustum* of an hexagonal pyramid of stone, in the founding whereof great care was taken as to its solidity, and was detached from the floor for supporting the observer.

This Observatory at *Austhorpe* I esteem in the latitude of $53^{\circ} 47' 54''$ N. and $5^{\circ} 50''$ of time W. from *Greenwich*.

Table I. Observations of Mercury at his elongation Sept. 1786, with an Equatorial Micrometer.

Day, object, and wires.	Hour.	Time as taken by the clock.	Time reduced to min. & sec.	Reduced to the middle wire.	Mean of the wires.	Parts of the microm.	Micrometer reduced.
Sept. 23.	AM.	M. q. bea.				Rev.Pts.	Rev.Pts.
Merc. to wire <i>a</i>	5	24 3 5	24 47.5	26 34.8			
	A	25 3 14½	25 52.3	26 34.8			
Middle wire B	—	26 2 9½	26 34.7	26 34.7	26 34.7	B 28 85	S 0.74
	C	27 1 6	27 18	26 34.7	{ N. B. The telescope's center was pointed to horary circle Vl. 34½ Decl. N. 7° 48'.		
	D	28 1 15	28 22.5	26 34.6			
	PM.						
λ Ceti to B	9	15 1 27	15 28.5	15 28.5	15 28.4		
	C	16 0 23	16 11.5	15 28.3			
♂ Tauri to B	—	40 1 25	40 27.5	40 27.5	40 27.4	B 8.39	N 19.72
	C	41 0 21	41 10.5	40 27.3			
Sept. 26.							
λ Ceti to <i>a</i>	9	2 2 13	2 36.5	4 23.5			
	A	3 2 23	3 41.5	4 23.9	4 23.8	B 16.97	N 11.14
	C	5 0 14½	5 7.3	4 24.1			
♂ Tauri to A	—	28 2 21	28 40.5	29 22.9			
	B	29 1 16	29 23	29 23	29 23	B 8.47	N 19.64
Sept. 30.							
λ Ceti to <i>a</i>	8	47 0 3½	47 1.8	48 48.8			
	A	48 0 13	48 6.5	48 48.9			
	B	48 3 8	48 49	48 49	48 49	B 16.97	N 11.14
	C	49 2 4½	49 32.3	48 49.1			
♂ Tauri to A	9	13 0 11	13 5.5	13 47.9			
	B	13 3 6	13 48	13 48	13 48.1	B 8.48	N 19.63
	C	14 2 3	14 31.5	13 48.3			
♂ Orionis to <i>a</i>	11	41 3 12	41 51	43 38			
	A	42 3 20½	42 55.3	43 37.7			
	B	43 2 17	43 38.5	43 38.5	43 37.9	A 15.07	S 15.77
	C	44 1 12	44 21	43 37.8			
	D	45 1 20½	45 25.3	43 37.7			
Oct. 13.							
λ Ceti to A	7	58 3 0	58 45	59 27.4			
	B	59 1 25½	59 27.7	59 27.7	59 27.5	B 16.97	N 11.14
	C	0 0 21½	0 10.7	59 27.5			
♂ Tauri to C	—	25 0 20	25 10	24 26.8	24 26.8	B 8.50	N 19.61
♂ Orionis to <i>a</i>	10	52 2 0½	52 30.2	54 17.2			
	A	53 2 9	53 34.5	54 16.9			
	B	54 1 4½	54 17.2	54 17.2	54 17.1	A 15.07	S 15.77
	C	55 0 0½	55 0.2	54 17			
	D	56 0 10	56 5	54 17.4			
1	2	3	4	5	6	7	8

Table II. For reducing the horary wires of the Equatorial Micrometer to that of the middle, when taken in mean solar time.

	Wires.	Equatorial object.		Declination 7° 48'	
		☉'s run	*'s run	☉'s run	*'s run
The 1st wire precedes the middle, add	a	1' 46.2	1' 46	1' 47.3	1' 47
2d —————	A	0 42.1	0 42	0 42.5	0 42.4
3d, or middle wire — —	B	— —	— —	— —	— —
4th is subsequent to the middle, subtract	C	0 42.9	0 42.8	0 43.3	0 43.2
5th —————	D	1 46.8	1 46.6	1 47.9	1 47.6
I	2	3	4	5	6

Table

Table III. Containing the observations of Tab. I. reduced so as to shew the correct differences of right ascension and declination between Mercury and the stars wherewith he was compared.

1786. Date and object.	Hour.	Passage over mid. hor. wire by journ. clock.	Correction to reduce the clock to mean time.	Correct mean time of the observation.	Intervals of mean time of different observations.	Parts of microme- ter from the tele- scope's center.	Pts. of micr. reduced= declin. from the tele- scope's center.
Sept. 23. γ to the mid. wire	AM 5	26 34.7	-3 59.8	h. 5 22 34.9		Rev. Pts. S 0.74	8 4 8
α Ceti to mid. wire	PM 9	15 28.4	-4 0.1	9 11 28.3	h. 15 48 53.4		
β Tauri to the same	9	40 27.4	-4 0.1	9 36 27.3	0 24 59	N 19.72	N 30 26
Sept. 26. α Ceti to mid. wire	9	4 23.8	-4 43.2	8 59 40.6		N 11.14	N 17 11
β Tauri to the same	9	29 23	-4 43.2	9 24 39.8	0 24 59.2	N 19.64	N 30 18
Sept 30. α Ceti to mid. wire	8	48 49	-4 50.9	8 43 58.1		N 11.14	N 17 11
β Tauri to the same	9	13 48.1	-4 50.9	9 8 57.2	0 24 59.1	N 19.63	N 30 17
α Orion. to the same	11	43 37.9	-4 50.8	11 38 47.1	2 29 49.9	S 15.77	S 24 20
Oct. 13. α Ceti to mid. wire	7	59 27.5	-6 36.4	7 52 51.1		N 11.14	N 17 11
β Tauri to the same	8	24 26.8	-6 36.8	8 17 50	0 24 58.9	N 19.61	N 30 15
α Orion. to the same	10	54 17.1	-6 37	10 47 40.1	2 29 50.1	S 15.77	S 24 20
1	2	3	4	5	6	7	

Table

Table IV. Deviations in the direction of the axis of the telescope of the Equatorial Micrometer in right ascension and declination in 20 days, from the 23d of September to the 13th of October, both inclusive.

Objects observed.	3 days from the 23d to 26th.	4 days from the 26th to 30th.	7 days from the 23d to 30th	13 days from the 30th to 13th.	17 days from the 26th to 13th	20 days from the 23d to 13th.
α Ceti { R. Ascension Declination	exact " O not taken the 23d	too late 1.1 exact O.	too late 1.1 not taken the 23d	too soon 0.3 exact O.	too late 0.8 exact O.	too late 0.8 not taken the 23d
δ Tauri { R. Ascension Declination	too late 0.2 South 8.	too late 1.0 South 1.	too late 1.2 South 9.	too soon 0.5 South 2.	too late 0.5 South 3.	too late 0.7 South 11.
α Orionis { R. Ascension Declination	- - -	- - -	- - -	too soon 0.3 exact O.	- - -	- - -

N. B. The right ascension is expressed in the integer and decimal parts of a second of time. The declination is expressed in seconds of a degree.

Explanation of the less obvious parts of the Tables of the Observation of Mercury near his Elongation, Sept. 1786.

The third column of Tab. I. contains the times of observation as they were taken down from the half-second *journeyman* clock, in minutes, quarters, and *beats*, according to the following method; which was, by taking up the beat when the second hand came to 15, 30, 45, or 60, and then counting 30 beats repeatedly till the arrival of the object at the middle of the wire it was approaching; after its arrival, the beats (or interval between two beats) being retained in memory, and the eye cast upon the dial-plate, it was easily seen whether it was so many beats more than the quarter, the half, three-quarters, or the whole minute, and was set down accordingly. Those reduced to minutes, seconds, and tenths of seconds, by allowing .2 or .3 for the quarter second, .5 the half, and .7 or .8 for the three-quarters of a second, are contained in the fourth column. The reduction of the fourth column to the fifth was by means of the auxiliary Tab. II.; and Mercury being then nearly stationary respecting the sun, the sun's run was used for the planet instead of that of a star. The mean of each set of observations of the fifth column is carried into the sixth.

The seventh column contains the parts of the micrometer as they were read off; to render which intelligible, it is to be noted, that the declination wire *A* travels from the upper side of the field of view of the telescope towards the center, and somewhat beyond it: and upon it are taken all the objects that pass the field of view on the upper side, answerable (by inversion of the object) to the southern half of the field: and in like manner those that pass the field of view on the lower half

half are taken upon the wire *B*, and for the same reason denote a declination north. The scale of the micrometers of each wire begins from a point assumed somewhat without the field, and the number increases from thence towards the center of the field, and continues beyond it; the integral parts are the turns of the screw, and the centesimal the divisions of the index plate, being divided into 100 parts. The point of the scale, answerable to the center of the field of view, having been found by observations on each scale respectively; when the wire *A* (*Australis*) stands at 30.84, it is in the center of the field; and when the wire *B* (*Borealis*) is at 28.11, it also cuts the same center. Hence the parts of the micrometer being respectively taken from those two numbers (which may therefore be called *constant* numbers) the remainder will be the distance of each respective wire from the center in parts of the micrometer. Thus, in the observation of σ Tauri upon the 23d, the parts are *B* 8.39; this taken from 28.11, leaves *N* 19.72, which are placed in col. 8. as the distance, in parts of the micrometer, that σ Tauri passed north of the center of the field of view, or axis of the telescope.

In like manner, in the observation of Mercury on the 23d, the parts are *B* 28.85; but this being greater than the constant number 28.11, the excess will be .74 parts; which being the parts reaching beyond the center, they will be so much *south* of it, and are set down therefore in col. 8. *S* 0.74: and in this manner the declinations of the rest are made out, from their respective numbers of parts of the micrometer, and set down in col. 8.

The numbers of the sixth column of Tab. I. are transferred to the third column of Tab. III.; and the declinations set down in parts of the micrometer, Tab. I. col. 8. are transferred to col. 7. of Tab. III.

Col. 4. of this table contains the corrections of the times deduced from the journeyman clock (as *per* col. 3.) to reduce it to mean time; which corrections are made out from the general account of the goings of the transit clock, corrected by transits of the sun, taken the 22d, 23d, 27th, and 30th of September, and the 12th, 13th, and 14th of October *. The journeyman clock was regularly compared at nights and mornings with the transit clock; and generally immediately after the observation. The *meridian* and *rotative* observatories in which the clocks respectively were, are at the distance of 53 yards E. and W.; the comparisons were made by a seconds stop watch †.

The numbers of the fourth column being properly applied to those of the third produce the fifth; and which, with the sixth column, will be sufficiently explained by their titles. The parts of the micrometer in the seventh column, being reduced into minutes and seconds, are contained in col. 8. and respectively shew the minutes and seconds at which each object passed to the north or south of the center of the telescope. The value of the parts of the micrometer were obtained by previous observations, from whence the following rule was deduced: the numbers of turns and centesimal parts being considered as integral, and divided by 1.08, the quotient will be the number of seconds. Thus, in the observation of *♂ Tauri*

* The transit clock was made by HINDLEY, and has a pendulum rod of cedar wood.

† The journeyman clock was generally set to the transit clock on Sunday mornings; and when from home the former was suffered to go down. The journeyman will *generally* agree with the transit clock to 2" in 24 hours; but during the period of these observations, went remarkably well.

upon

upon the 23d, the parts 1972, divided by 1.08, gives $1826'' = 30' 26''$; and the parts of mercury :74, divided by 1.08 = $68'' = 1' 8''$. Now the telescope being fixed to one point of the heavens during the whole period of these observations, without any motion of any of the parts, the screws commanding the declination wires *A* and *B* excepted, we are enabled to judge of its steadiness to this point by the following remarks. If it varied in declination, this would be shewn by the passage of the same star at a different distance from the center of the telescope at different revolutions; and if it varied in right ascension, it would be shewn by its not passing the horary wires at the due time, according to the acceleration of the stars upon the mean time of the sun. Both the right ascension and declination may be varied by differences of refraction of the air at the same altitude; and the right ascension is further liable to be *apparently* varied, by the errors of the transit instrument, the transit clock, the transferring of its time to the journeyman clock, the intermediate errors of the same, and of the observation itself; and as, there passed an interval of almost 16 hours betwixt the passage of Mercury over the field of view of the telescope and that of λ Ceti, which was the nearest star wherewith a comparison could be made, it will be a satisfaction to see, as before intimated, what variations arose in still greater intervals of time.

In right ascension.

	h.	9 11 28.3
Thus λ Ceti upon Sept. 23. passed the horary wires at		
and _____ 26. _____		8 59 40.6
λ Ceti therefore came sooner in three days by		11 47.7
but _____ ought to accelerate on mean time		11 47.7
_____ therefore came after three days exactly to the time.		

Again...

Again, α Tauri upon Sept. 23. passed the horary wires at	-	-	h.	9	36	27.3
and _____ 26. _____	-	-		9	24	39.8
α Tauri therefore came sooner after three days by	-	-		11	47.5	
_____ ought to accelerate on mean time	-	-		11	47.7	
_____ therefore came too late in three days by	-	-				.2

In declination.

α Tauri upon Sept. 23. passed north of telescope's center	-	-		30	26	
_____ 26. _____	-	-		30	18	
_____ therefore passed less north, or more south, than before by	-	-				8

In like manner every comparison that Tab. III. affords is particularly set down in Tab. IV. which containing thirteen comparisons in right ascension and ten in declination, the greatest deviation in right ascension is $1''.2$, and $11''$ of a degree in declination. This supposes every error before mentioned to reside in the instrument, and every other instrument and observation, which were concerned in the result, to be perfect; which, from the smallness of the total errors, seems to indicate a degree of steadiness in the instrument unexperienced or unnoticed before.

Deduction of the position of Mercury from the preceding observations as set down in Tab. III.

In right ascension from col. 6.

α Tauri followed λ Ceti Sept. 23.	-	-	-	24	59	
_____ 26.	-	-	-	24	59.2	
_____ 30.	-	-	-	24	59.1	
_____ Oct. 13.	-	-	-	24	58.7	
_____ at a mean of the four	-	-	-	24	59	
						α Orionis

with an Equatorial Micrometer.

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α Orionis followed \circ Tauri Sept. 30.	—	—	—	h.	'	"
_____ Oct. 13.	—	—	—	2	29	49.9
_____ at a mean	—	—	—	2	29	50.1
				2	29	50

Now Mercury preceded λ Ceti Sept. 23.	—	—	—	15	48	53.4
λ Ceti preceded \circ Tauri by mean of four	—	—	—	24	59	
\circ Tauri preceded α Orionis by mean of two	—	—	—	2	29	50
Mercury therefore preceded α Orionis by	—	—	—	18	43	42.4

In declination from col. 8.

Sept. 23. A.M. Mercury passed the middle horary wire, south of its center $1^{\circ} 8'$
 Same evening \circ Tauri passed the middle horary wire, north of it $30^{\circ} 26'$

Therefore Mercury passed the middle horary wire more S. than \circ Tauri by $31^{\circ} 34'$

But Sept. 26. λ Ceti passed N. of center	17	11	} Diff. 13 7
_____ \circ Tauri _____	30	18	
_____ 30. λ Ceti _____	17	11	} — 13 6
_____ \circ Tauri _____	30	17	
_____ Oct. 13. λ Ceti _____	17	11	} — 13 4
_____ \circ Tauri _____	30	15	

From the smallness of the above differences we may infer, that very little uncertainty in declination had attended the passage of \circ Tauri upon Sept 23.

Upon Sept. 30. \circ Tauri passed N.	30	17	} Sum. 54 37
_____ α Orionis _____ S.	24	20	
Upon Oct. 13. \circ Tauri passed N.	30	15	} — 54 35
_____ α Orionis _____ S.	24	20	

α Orionis then at a mean passed more south than \circ Tauri $54^{\circ} 36'$

Merc. therefore on the 23d passed with more N. declination than α Orionis $23^{\circ} 2'$

Investigation

Investigation of the effects of refraction.

The preceding deductions and remarks shew the consistency of the observations with themselves; yet, from the position of the telescope, it being only elevated $11^{\circ}\frac{1}{2}$ above the horizon *, it is necessary to examine how far the deductions above specified were capable of being affected by refraction. And in this respect it will appear, that if it be supposed, there is no difference in the quantity of refraction of such objects as appear within the limits of the field of view of this instrument (which is $1^{\circ} 17'$), then their relative positions to each other will not be affected thereby: for if in fig. 1. (Tab. XIII.) we suppose the circle VHRO to represent the boundary of the field of view, HO being an horizontal and VR a vertical line, each passing through the center of the field at L; and if PLP denotes a part of a parallel of declination, then BLX perpendicular thereto, will be a part of an horary circle, both passing through the same center. Now let $d*$ be the apparent path of a star, supposing it unaffected by refraction till it comes to the vertical line at *, and there to be lifted up by refraction in the said vertical to L. Let $e+$ denote another star, also unaffected by refraction, to pass along the different parallel of declination $e+$ till it comes to +; then, if it be supposed that the two stars are both situated in the same horary circle, if at the point + refraction takes place, and by hypothesis this is lifted up equally with the other, in the perpendicular +L, then the line +* being drawn through the places of the two stars, will be cotemporary and parallel to LX; and the figure L+*L being evidently a rhomboides, the two stars, so altered by

* This will readily be deduced by inspection of the celestial globe.

refraction,

refraction, will arrive together at the horary circle LX at the same time, and with the same difference of declination, as if no refraction had taken place. It is therefore only the *difference* of refraction which takes place in objects at different heights in the *same field*; that can alter their relative situations: however, it appears necessary to examine what this may amount to.

Let the letters in fig. 2. denote the same things as before; to which we will add, that a, A, B, C, D , denote the parallel horary wires of the micrometer, and AA, BB , the declination wires, denoted A and B in the tables: now from the celestial globe we shall also readily obtain the horary angle $VLP = 54^{\circ}\frac{1}{2} = Lbc$. Let now an object pass along the wire AA from the horizontal line at d to the vertical line at b ; in this it will pass through a difference of refraction, according as it gets more and more elevated above the horizontal line HO; and let the elevation Lb be half a degree or 30 minutes: then, according to Dr. BRADLEY's Table of Refraction*, the difference of refraction betwixt the 78th and 79th degrees of zenith distance is $23''.6$, half of which $11''.8$, may be esteemed the difference of refraction for a difference of half a degree of altitude at $78^{\circ}\frac{1}{2}$ zenith distance, or of $11^{\circ}\frac{1}{2}$ altitude: the object, therefore, in passing from the horizontal line at d to the vertical line at b passes through every difference of refraction from $0''$ to $11''.8$; and the question is, how much it is at a *medium*, that is, when it arrives at the middle wire at the point c ? From this point let fall the perpendicular ce . Now, the proportion of the sides of the triangle dbL being given from construction, they may be taken off by a scale, *viz.*

* Inserted in Dr. MASKELYNE's Observations, Vol. I. p. 15.

Suppose $Lb = 174$

$db = 299$

$dL = 242$

and assuming the side $Lb = 30$

the other sides by proportion $\left\{ \begin{array}{l} db = 51.6 \\ dL = 41.7 \end{array} \right.$
as above will be

The triangles Lbc and dce are similar to dbL ; therefore say, as $db = 51.6 : dL = 41.7 :: Lb = 30 : Lc = 24$; and as $Lb = 30 : Lc = 24 :: dL = 41.7 : dc = 33.5$; and again, as $db = 51.6 : dc = 33.5 :: Lb = 30 : ce = 19.5$: but this will affect the declination, only in proportion of the line ef drawn parallel to LX ; and it will affect the right ascension according to the line fc : but the triangle ecf being similar to the original one dbL , we shall have $db = 51.6 : Lb = 30 :: ce = 19.5 : fc = 11.3$ for the line affecting the right ascension; and also, as $db = 51.6 : dL = 41.7 :: ce = 19.5 : ef = 15.8$ for the line affecting the declination. But the effect of difference of refraction upon the line $Lb = 30'$ being only $11''.8$, the respective effects of the lines fc and ef will be in proportion; that is,

as $30' : 11.3 :: 11''.8 : 4.4$ for the effect in right ascension,
and as $30 : 15.8 :: 11.8 : 6.2$ ————— declination;
but as it has been determined, that when the line Lb is 30 minutes, the line LC , or the corresponding declination, will be only 24 minutes; the effects of refraction above stated will be therefore due to $24'$.

Correction for the position of the wires.

The above corrections take place on supposition that the several wires of the micrometer were strictly parallel to the respective parts of the circles of declination, and horary circles in the heavens; but in the practical use of this instrument it
is

is found more convenient, on account of a ready and certain adjustment, to place one of the wires AA or BB parallel to the apparent track of the star wherewith the *planetary* body is to be compared: in consequence, when the star $*$, fig. 1. is lifted up to L , it will not strictly pursue the line LP ; but being less and less lifted up as it mounts higher, it will apparently fall more and more below the line LP as it ascends above the line HO , and will therefore take a course, suppose Lp . The wire PLP being therefore adjusted to agree with pLp ; by construction of the instrument, the wire BLX will assume the position qLx perpendicular to pLp . The star, therefore, that ran along the parallel $e+$ before it suffered refraction, and at $+$ was supposed to be lifted up to l , there not meeting LX will take the course ly , nearly parallel to Lp , and have some distance, as lz , to travel before it arrives at the new-placed wire Lx ; and it is now proper to examine what this quantity may be.

Through the point z draw the line $rxos$ parallel to HO , and cutting the vertical RLV in o , and let Lo be assumed $= 30'$; then, since the angle XLx is supposed to be *minute*, the *gross* proportions of the sides of the triangles Lyz and Lyl may be, for this purpose, supposed the same $*$, and the same as Lbc , dbL , fig. 2. to which the triangle Lzo , fig. 1. will also be similar; as likewise the triangle yzo , and also the little triangle z/v : but making the side lv of the triangle z/v equal to the effect of refraction in perpendicular $= 11''.8$; then, to find the side lz ,

* I am aware, that the supposition of the sides of the triangles Lyz and Lyl being the same cannot be strictly so; nor can they have the same proportions; nor are any of the lines concerned right lines, that are supposed such; but assumptions near the truth are allowable for the correction of an error in the *greatest part*, that if uncorrected would scarcely amount to a *gross error*.

C c c 2

the

the distance run from the first to the last supposed place of the wire, we need only say, as $Lb = 30 : db = 51.6 :: lv = 11''.8 : lz = 20''.3$; and this will be its value when the declination Lc , fig. 2. is $24'$; but then the declination Ll or Lz , fig. 1. being greater than the perpendicular side Lo (assumed $30'$) in the proportion of $Lz : Lo$, say, by similarity of triangles conversely, as $dL = 41.7 : db = 51.6 :: Lo = 30' : Lz = 38'.2$; but as the correction before stated of $20''.3$ is an angular error, taking place in proportion to the distance from the center, or the declination; for the declination given of $24'$ say, as $38.2 : 24' :: 20''.3 : 13$; to which adding $4''.4$, we shall have $17''.4$ for the whole error in right ascension, supposing it in the equator, but must be again increased in the proportion in which a star having declination is slower than a star in the equator; that is, it must be increased in the proportion of any of the numbers in the fourth column of Tab. II. to the similar ones in col. 6. of the same table; that is, as $1' 46'' : 1' 47''$ or as $106 : 107$, $:: 17''.4 : 17''.6$ *.

As all these errors, arising from difference of refraction, are in proportion of the distance of the object from the center of the telescope, they will take place in proportion to the difference of declination of the two objects to be compared; whether they have passed the field on the same, or on different sides of the center. Now the difference of declination of Mercury and α Orionis being only $23' 2''$, and the quantities being made out for $24'$ say (rejecting the 2 seconds), as $24' : 23' :: 17''.4 : 16''.7$, which turned into time in the run of the star will be $1''.1$ in right ascension.

* My friend Dr. MASKELYNE observes, that in *strictness* each star ought to have its own proper reduction, on account of *difference* of declination, which in *extreme cases* will amount to a sensible quantity.

Say again, as $24' : 23' :: 6''.2 : 6''$, the correction in declination. From the near equality of the lines $L/$ and Lz , it is evident, that no correction of *declination* is necessary on account of the inclination of the wires, the whole difference falling in right ascension. As therefore Mercury passed with $23' 2''$ more north declination than α Orionis, and passed through a part of the *medium* that lifted him up less; it therefore gave him less north declination than it did to α , and therefore apparently diminished the real difference; hence $6''$ must be added to the apparent difference $23' 2''$, making it $23' 8''$ difference of declination: and as Mercury was lifted up less than α , he would not so soon come to the middle wire by $1''.1$ as he should have done, he therefore came too late by $1''.1$, which must be subtracted from the time of Mercury's passage the 2d of Sept. which will increase the time in which he preceded α Orionis; that is, 18 h. $43' 42''.4$ increased by 1.1 will become 18 h. $43' 43''.5$ difference of right ascension.

I have been the more particular in the investigation of this observation, first of all to ascertain the degree of dependance that may be formed on an instrument of the kind; and, secondly, to infer such easy and simple rules, that other similar observations may be the more easily reduced. Being therefore satisfied of the stability of the instrument; if we had concluded the observation with that of Mercury in the morning, and of σ Tauri in the evening of the 23d, then the result from Tab. III. should have been

Mercury passed the wires at	-	-	-	h. 17 22 34.9
And σ Tauri passed at	-	-	-	9 36 27.3
Difference of right ascension	-	-	-	<hr/> 15 13 52.4 <hr/>
				which

which is the very same as was before deduced from the mean of the whole :

And if to Mercury's declination south of telescope's center	-	1' 8"
We add α Tauri's ————— north —————	-	30 26
		<hr/>
We shall have for the difference of declination	- -	31 34
		<hr/>

the same as before determined. Our observation would, therefore, in this case simply have been, that Mercury preceded α Tauri in right ascension 15 h. 13' 52''.4 mean time, and passed the wire with more south declination than α Tauri by 31' 34".

After this, α Tauri would have required to be compared with some well rectified star by meridian instruments; but in the present case α Orionis, one of Dr. MASKELYNE's Catalogue of 34 principal stars happened to lie sufficiently near the same parallel of declination, to admit of α Tauri to be compared therewith by the same instrument, while pointed to the same place of the heavens. The operations which were subsequent, therefore, must be considered as intended to save those of a meridian instrument.

Now had our observation concluded with the above, then the correction would have taken place upon the difference of declination of α Tauri with Mercury, instead of the ultimate one with α Orionis; but it must be observed, that whatever quantity of correction the difference of declination would occasion, it would be compensated in the difference of refraction of α Tauri and α Orionis, when they came to be observed on the meridian; however, in the present case it happens to be more commodious, as both can be done under one.

Preparatory then to the laying down the simple rule for the correction of refraction, it is proper to premise, that it is evident,

dent, the lines, fig. 2. Lb , Lc , ce , ef , being in continued proportion Lb will be to ef in triplicate proportion of Lb to Lc ; and that Lc will be to ef in duplicate proportion of $Lb : Lc$. The difference of declination, therefore, due to $30'$ difference of elevation will be as Lb to Lc simply; but the effect of difference of refraction in declination will be less than the difference of declination in the proportion of $Lb^2 : Lc^2$; and that the effect of difference of refraction in right ascension will be less than the difference of refraction in declination in the proportion of $Lb : cb$ simply.

Now it has been remarked, that the elevation of the telescope's center above the horizon, and the horary angle VLP , will always be readily given near enough for the purpose by the globe. A triangle given Lbd can therefore be constructed, and the side Lb being made $30'$ (or any convenient aliquot part of a degree) the other sides will be found by proportion: say then, as in the present case, $db = 51.6 : dL = 41.7 :: Lb = 30 : Lc = 24$, for the difference of declination corresponding to half a degree of altitude: say then, as $51.6^2 : 41.7^2$, that is, as $2663 : 1739 :: 24 : 15.7 = ef$. But without troubling ourselves with high numbers, if we take the proportion 51.6 to 41.7 by the slide-rule *twice*, we shall arrive at 15.7 , near enough for the value of the line ef : say then, as $Lb = 30 : ef = 15.7 :: 11''.8 : 6''.2$ for the refraction in declination: and as $dL = 41.7 : Lb = 30 :: 6''.2 : 4''.4$ for the refraction in right ascension, according to the *true* position of the wires: and, for the correction of right ascension in the position of the wires, say,

Fig. 2.	Fig. 1.
As $Lb = 30 : db = 51.6 ::$	$lv = 11''.8 : lz = 20''.3 ;$
and again, $dL = 41.7 : db = 51.6 ::$	$Lo = 30' : Lx = 38'.2.$

Take

Take now Lb , fig. 1. = Lc , fig. 2. = $24'$, and draw the line bik , fig. 1. parallel to the line lxy , and then say, as $Lz = 38'.2 : Lb$ (or Li) = $24' :: lz = 20''.3 : bi = 13''$, which + $4''.4 = 17''.4$ for the whole error in right ascension, with a declination or distance from the center of $24'$; but as the errors both of right ascension and declination are in proportion to distance from the center, as the difference of the planet and star is only $23'$, say, as $24' : 23' :: 17''.4 : 16''.7 = 1''.1$ time; and for the declination, say again, as $24' : 23' :: 6''.2 : 6''$ declination *.

Reduction of Mercury's comparison with α Orionis, to right ascension and declination.

We have laid it down, that the 23d Sept. 1786, A.M. at 5 h. 22' 34''.9 mean time, Mercury preceded α Orionis 18 h. 43' 43''.5, and had then a more northern declination by 23' 8''.

According to Dr. MASKELYNE's Catalogue of 34 stars, the right ascension of α Orionis reduced to the time when he was observed is $85^\circ 54' 12''$.

Now as the whole circle of the sphere makes a revolution in the time that α Orionis makes one turn, which is

$$\begin{array}{r} \text{h.} \\ 23 \ 56 \ 4.1 \text{ then from this deduct} \\ 18 \ 43 \ 43.5 \\ \hline \end{array}$$

5 12 20.6 remains for the time that α Orionis preceded

* If the comparison had been with ϵ Tauri, then we must have said,

$$\begin{array}{l} \text{As } 24' : 31\frac{1}{2} :: 17.5 : 23 = 1.5 \text{ of time,} \\ \text{and } 24' : 31\frac{1}{2} :: 6.2 : 8.1 \text{ correction for declination.} \end{array}$$

N. B. All these and the above proportions will be commodiously wrought with the slide-rule.

Mercury

Mercury in right ascension; but if α ran the whole rotation $= 360^\circ$ in 23 h. 56' 4''.1, what portion of it will be run in 5 h. 12' 20''.6 $= 18740''.6$?

But 24 h. $= 86,400$ seconds, and $360 = 1,296,000$ seconds:

	Time.	Time.	Of degrees.	Of degrees			
Say then, as	86400''	: 18740''.6	:: 1296000''	: 281109''	=	-	78 5 9
But, according to Dr. MASKELYNE's select Catalogue, the right ascen-							
sion of α Orionis for Sept. 30, 1786, was, (which add)					-		85 54 12

The right ascension of Mercury at the time of observation was therefore 163 59 21

According to Dr. MASKELYNE's select Catalogue α Orionis had decli-
nation north, corrected for precession - - 7 21 8.8

The sum of aberration and nutation from *Connaissance des Temps* - + 8.4

The correct declination north of α Orionis - - 7 21 17.2

To which add that Mercury passed more north - - 23 8

Mercury's declination therefore was - - 7 44 25.2

The result.

1786, Sept 23. A.M.	} Mercury's {	right ascension	-	163 59 21
at 5 h. 22' 35'' M.T.		declination north	-	7 44 25



XXXIV. *A remarkable Case of numerous Births, with Observations.* By Maxwell Garthshore, M. D. F. R. S. and A. S. in a Letter to Sir Joseph Banks, Bart. P. R. S.

Read June 21, 1787.

TO SIR JOSEPH BANKS, BART. P. R. S.

S I R,

St. Martin's-Lane. May 28, 1787.

THE following very extraordinary case, communicated to me by Dr. BLANE, F. R. S. I take the liberty, at his desire, to transmit to you, with his letter to me, containing the proofs of its authenticity; hoping that it will appear to you, as it did to us, worthy of being read at one of the meetings of the Royal Society, as a fact in natural history, which is equally uncommon, curious, and well vouched. In order, however, to make its singularity more apparent, I have taken the liberty to subjoin some observations on births of this kind, with such well authenticated accounts of similar events as I have been able to procure, confining myself chiefly to those which have happened in our own country, where we are least likely to be deceived.

I have the honour to be, &c.

MAXWELL GARTHSHORE.

P. S. As one proof of its singularity, I, many months ago, employed various friends at Petersburg, Berlin, Vienna, Lyons, Paris, and Ghent. to collect for me well authenticated cases of this kind, and I have not as yet been able to procure any.

Copy of a letter from Dr. BLANE, Physician to his Majesty's Navy and to St. Thomas's Hospital, F. R. S. to Dr. Garthshore, Physician to the British Lying-in Hospital.

DEAR SIR,

Sackville-Street, June 22, 1786.

A few days ago, I received from the country an account of a woman who was delivered of five children at a birth in April last. As your extensive experience and reading in this line of practice enable you to judge, how far this fact is rare or interesting, I submit it to you, whether it deserves to be communicated to the Royal Society. Mr. HULL, the gentleman who sent me the case, is a very sensible and ingenious practitioner of physic at Blackburn, in Lancashire. He attended the labour himself from beginning to end, and his character for fidelity and accuracy is well known to me, as he was formerly a pupil at the hospital to which I am physician; so that no fact can be better authenticated. He mentions also, that he has preserved all those five children in spirits; and, if desired, he will send them for the inspection of the Society*.

I am, with great regard, &c.

GILBERT BLANE.

* They were accordingly sent; and having been exhibited to the Society when this Paper was read, are now deposited in the Museum of Mr. JOHN HUNTER.

MARGARET WADDINGTON, aged twenty-one, a poor woman of the township of Lower Darwin, near Blackburn in Lancashire, formerly delivered of one child at the full term of pregnancy, conceived a second time about the beginning of December 1785, and from that period became affected with the usual symptoms that attend breeding. At the end of the first month, she became lame, complained of considerable pains in her loins, and the enlargement of her body was so remarkably rapid, that she was then judged by her neighbours to be almost half gone with child. At the end of the second month she found herself somewhat larger, and her breeding complaints continued to increase. When the third month was completed, she thought herself fully as large as she had formerly been in her ninth month, and to her former symptoms of nausea, vomiting, lameness, and pain of the loins, she had now added a distressing shortness of breath. She continued to increase so rapidly in size, that she thought she could perceive herself growing larger every day, and she was under the frequent necessity of widening her cloaths. When she reckoned herself eighteen weeks gone, she first perceived somewhat indistinctly the motion of a child. By the 20th of April, 1786, all her complaints were become much more distressing; she had much tension and pain over all the abdomen, her vomiting was incessant, and she now could not make water but with the utmost difficulty. The symptoms being palliated by Mr. LANCASTER, she advanced in her pregnancy to Monday the 24th of April, when being supposed to have arrived at the twentieth week, she was seized with labour pains. These continued gradually to increase till the next day, about two in the afternoon; at which time I was sent for, Mr. LANCASTER being absent, and

and she was soon delivered of a small, dead, but not putrid, female child. The pains continuing, this was soon followed by a second less child; to this very soon succeeded a third, larger than the first, which was alive; to these a fourth soon followed, somewhat larger than the first, and very putrid; last of all, there soon succeeded a fifth child, larger than any of the former, and born alive. These five children were all females; two were born alive; and the whole operation was performed in the space of fifty minutes. The first made its appearance at two in the afternoon, and the last at ten minutes before three. Each child presented naturally, was preceded by a separate burst of water, and was delivered by the natural pains only. In a short time after the birth of the last, the placenta was expelled by nature without any hæmorrhage, was uncommonly large, and in some places beginning to be putrid. It consisted of one uniform continued cake, and was not divided into distinct placentulæ, the lobulated appearance being nearly equal all over. Each funis was contained in a separate cell, within which each child had been lodged; and it was easy to perceive, by the state of the funis, and that part of the placenta to which it adhered, in which lay the dead, and in which the living children had been contained. I examined the septa of the cells very carefully, but could not divide them as usual into distinct laminæ, nor determine which was chorion or which amnios. I could not prevail on the good women to allow me to carry it home, to be more narrowly inspected; and I submitted more readily to their prejudice for its being burned, as its very soft texture seemed to me to render it hardly capable to bear injection. The two living children having survived their birth but a short time, I was allowed to carry them home; and I have preserved the whole five in spirits, and have
since

since weighed and measured them, and find their proportions to be as follows in Avoirdupois weight, inches and parts.

		Oz. Dr.	Length	Inches.
The 1st born dead	- -	6 12		9
The 2d ——— putrid	- -	4 6		8½
The 3d ——— alive	- -	8 12		9½
The 4th ——— putrid	- -	6 12		9½
The 5th ——— alive	- -	9 —		9½

The mother, in spite of the crowds with which her chamber was continually filled, continued to recover, and was able to be out of bed on the 27th and 28th, her third and fourth days; but finding herself then weak, by my advice, kept her bed till the 11th of May, when she went out of doors, and on the 21st walked to Blackburn, two miles distant. This was the 27th day from her delivery, she having entirely recovered her strength without any accident. It may not be improper to add, that the husband of this woman has been in an infirm state of health for three years past, and is now labouring under a confirmed phthisis.

I am, &c.

Signed, JOHN HULL

Blackburn, Lancashire,
June 9, 1786.

Observations

Observations on numerous Births.

THOUGH the females of the human species produce most commonly but one child at a birth; and though their formation with only two breasts, and one nipple to each, renders it probable they were not originally intended to produce in general more than two; yet, from what we know of the womb and its appendages, and what from the latest experiments we are led to conjecture as to the mode of conception, we cannot presume *à priori* to set limits to the fertility of nature, nor determine decisively what number of fœtuses may be conceived and nourished to a certain period in the human uterus at the same time.

The present singular and well-attested case assures us, that five have certainly been born at once, and we have no title absolutely to reject all the testimonies of even more numerous births, or to say that, in some rare instances, this number has never been exceeded.

What has tended to render relations of this sort ridiculous, and to throw a degree of discredit on the whole, is the many marvellous, and evidently absurd and incredible histories, which not only the retailers of prodigies, but even the credulous writers of medical observations, have collected.

I need only refer those, who wish to amuse themselves with surprising relations of this kind, to the curious collections of SCHENKIUS, SCHURIGIUS, AMBROSE PAREY, and others.

But, in order to shew how very uncommon births of this kind are, and how truly singular the case communicated by Mr. HULL to Dr. BLANE is, I take the liberty to subjoin a
short

short view of the usual course of nature in this matter among our own country-women, where we are least likely to be deceived.

Though female fertility certainly varies according to the climate, situation, and manner of life; yet, I believe, it may be taken for a general rule, that where people live in the most simple and natural state, if they are the best nourished, and if they enjoy the firmest health and strength, they will there be the most fertile in healthy children; but we have no *data* to determine that they will there have the greatest number at one birth.

At the British Lying-in Hospital, where we have had 18,300 delivered, the proportion of twins born has been only one in 91 births. In the Westminster Dispensary, of 1897 women delivered, the proportion of twins has been once in 80 births; but in the Dublin Lying-in Hospital, where above 21,000 have been delivered, they have had twins born once every sixty-second time.' The average of which is once in 78 births nearly, in these kingdoms.

The calculations made in Germany from great numbers, in various situations, state twins as happening in a varied proportion from once every sixty-fifth to once every seventieth time.

But in a more accurate and later calculation made at Paris, by M. TENON, Surgeon to the Salpêtrière, we learn, that in 104,591 births the proportion of twins was only one in 96, which is only a small degree less than we have calculated at the British Lying-in Hospital.

It would be easy to add other calculations, all differing from these and from one another, more or less; but I hope these are sufficient to shew that nature observes no certain rule in this matter;

matter; and that even twins, the most usual variation, is not a very common occurrence.

When we advance to triplets, or three born at once, we find comparatively very few instances in this or any other country; and though every one has heard of such events as now and then happening, yet very few have seen them.

In all those 18,300 women delivered at the British Lying-in Hospital, there has not been one such case. In the London Lying-in Hospital, where, being instituted later, much fewer have been delivered, they have had two such recorded as prodigies. In the Westminster Dispensary, in 1897 women delivered, there has been but one such event.

In the Dublin Hospital, in 21,000 births, they have had triplets born thrice, or once in 7000 times, but have never exceeded that proportion or number, born at one time.

In a pretty extensive practice of above thirty years, both in the county of Rutland and in London, I have attended but one labour where three children were born; am personally acquainted but with one lady who, at Dumfries, in Scotland, after bearing twins twice, was delivered of three children at once; and I was never acquainted with any one who produced a greater number.

Yet so much does this matter vary at Edinburgh, that Dr. HAMILTON, Professor of Midwifry, writes, he had seen triplets born there, five or six times in less than twenty-five years.

MAURICEAU, in a long life of very extensive practice at Paris, with opportunities of knowing most things extraordinary that happened in his time in France, tells us, he had seen triplets born but a few times; had heard of four in that city but once, and mentions no greater number.

One circumstance which he relates is so far worthy of attention, as it accords with one somewhat similar subjoined to Mr.

HULL's case now read, *viz.* "That the husband of one of those women who bore three children was by trade a painter, and had been, for two years preceding this birth, paralytic over one-half of his body, and yet had no reason to doubt the fidelity of his wife."

These facts, as far as they are to be depended on, may shew us, that the capacity of procreation in the male may remain under very infirm health; and that we ought to judge with candour of such wives as are fruitful when living with very ailing husbands, and who produce healthy children in the eighth, or even ninth, month after their death; as we can never say determinately under what degree of disease the male is totally incapable of procreation: more especially as we are very certain, that the female is not, when labouring under very desperate, and certainly fatal, diseases, provided the principal organs of generation be sound. Nay, in cases of pulmonary phthisis, the life of the female seems to be protracted by pregnancy; and I have attended a lady, who, after being pronounced irrecoverably hectic, lived long enough to be twice delivered naturally of healthy children at the full time.

But what particular circumstances of constitution, or state of health, can capacitate the male to become the father of more than one child at a birth, or how this could be effected, should it be wished, remains among those secrets of nature which our want of facts and observations renders us utterly incapable to speculate upon.

It seems probable, and these two observations, as well as SPALLANZANI's, and other late experiments, would rather incline us to suppose, that these numerous births do depend most on the structure and state of the female organs; but
nothing,

nothing, that I know of, has ever been discovered in this obscure matter.

The occurrence of four born at once we find to be much more uncommon; and, I think, HALLER's conjecture rather than calculation of its happening once in 20,000 births, very much under-rated, as it appears that once in 100,000 would be much nearer the truth. Of this, however, we have several well authenticated cases which have happened in this island. In the year 1674, there was published in London a quarto pamphlet, intituled, "The fruitful Wonder, or a strange Relation, from Kingston upon Thames, of a Woman who, on Thursday and Friday, the Fifth and Sixth Days of this Instant March, 1673-4, was delivered of Four Children at one Birth, viz. Three Sons and One Daughter, all born alive, lusty Children, and perfect in every Part, which lived Twenty-four Hours, and then died, all much about the same Time, with several other Examples of numerous Births, from credible Historians, with the Physical and Astrological Reasons for the same. By J. P. Student in Physic."

Dr. PLOTT, in his History of Staffordshire, p. 194. mentions ELEANOR, the wife of HENRY DIVEN, of Watlington, who was delivered of four children at a birth in the year 1675.

Sir ROBERT SIBBALD, in his *Scotia Illustrata*, after mentioning a case of three born at once, adds, "Imo in variis regni locis repertæ sunt mulieres quæ quatuor foetus uno partu ediderunt;" but makes no mention of more.

In the Gentleman's Magazine, which is reckoned a pretty authentic record of the times, we have the following accounts of numerous births.

ANN BOYNTON, of Hensbridge, in Somersetshire, was this day, June 1, 1736, delivered of three daughters and one son; one of the daughters died, the rest are likely to live. The mother has been married but four years, and has had twice twins before, which completes the number of eight children at three births.

October 3, 1743, at Rate, in Berkshire, JOAN GALLO-WAY was delivered of two boys and two girls, three of whom were alive.

In January, 1746, the wife of PLUMER, a labouring man, at Mill-Wimley, near Hitchin, Hertfordshire, was delivered of three living boys and one dead.

August 22, 1746, the wife of WILLIAMS, of Coventry-street, Piccadilly, was delivered of two boys and two girls, all likely to live.

June, 1752, a woman in the parish of Tillicultrie, near Stirling, in Scotland, was delivered of four children, which were all immediately baptised, and all died at the same time next morning.

In September, 1757, a poor woman, of Burton Ferry, Glamorganshire, was delivered of three boys and a girl.

Dr. HAMILTON before mentioned writes, that, not many years ago, a woman was delivered of four children, at Penny-cuick, the seat of Sir JOHN CLARK, Bart. near Edinburgh, when she was advanced to the middle of her last month of pregnancy, and that some of these children lived two or three years. He further says, that, five years ago, he attended a woman at Edinburgh, who, in the seventh month of her pregnancy, after a journey of thirty miles, was suddenly delivered of four children, all perfect and well grown for the time, of which one was born dead, and three alive; but those three

died next day. He further adds, that these are the only cases of quadruplets, or any larger number, he had ever heard of, as born in Scotland, in his memory.

Though cases similar to the present of five children born at once, are still much more uncommon; and though HALLER's assertion of their not happening above once in a million of births, may be reckoned a very moderate calculation, yet we are not altogether without such instances in this country.

From the Gentleman's Magazine we learn, that on the 5th of October, 1736, a woman at a milk-cellar, in the Strand, was delivered of three boys and two girls at one birth; and that in March, 1739, at Wells, in Somersetshire, a woman was delivered of four sons and a daughter, all alive, all christened, and all then seeming likely to live.

In the *Commercium Literarium Norimbergense* for the year 1731, we have two such cases; one happening in Upper Saxony, the other near Prague, in Bohemia; in each of which five children were born and christened, all of whom were arrived to that equal degree of maturity, which rendered it probable, they were all conceived about the same time.

I learned from two foreign Professors, when in London last winter, that they had each heard of a case of five children born near Paris, and near Ghent in Flanders; but the particulars not being sent as promised, I presume they may have been misinformed.

When we advance farther we get into the region of tradition and improbability; and it would ill become me to trouble a Society, whose professed object is truth and science, with the numerous and wonderful relations which many grave and learned authors have recorded as facts they themselves believed; yet I still think we have no authority to reject absolutely every relation

relation of this kind, when AMBROSE PAREY, a very honest though credulous man, tells, that in his time, in the parish of Sceaux, near Chambellay between Sarte and Maine, the mother of the then living lord of the noble house of MALDEMEURE had, in the first year of her marriage, brought forth twins, in the second triplets, in the third four, in the fourth five, and in the fifth year six children at one birth, of which labour she died; and when he adds, that of these *last six* one is yet alive, and is now Lord of Maldemeure, how can we disbelieve this circumstance? This story may very possibly be inaccurately stated, yet the whole cannot be a fiction, as it was published among the very people, and in the age when it happened, and never has been since contradicted so far as we know. Though the wonderful regularity of the progress gives an appearance of fable to the whole, yet we must believe the thing to be possible: and that this then existing lord might be the only one of the six who lived long enough to be born at the full time, in a mature state; the whole, or most of the other five, as we have sometimes seen in cases of twins, having been born as dead abortions, which had never arrived to a bulk sufficient to interfere with his growth.

I leave the learned to pay what degree of credit they please to the wonderful relations we read of the extreme fertility of the women of Egypt, Arabia, and other warm countries, as recorded by ARISTOTLE, by PLINY, and by ALBUCASIS, where three, four, five, and six children are said to have been frequently born at once, and the greatest part of these reared to maturity; and will only say, that though a late traveller M. SAVARY gives ample testimony of the extreme general fertility of Egypt in all vegetable and animal productions, and particularly

ticularly of its abundant population, he mentions nothing of the numerous births recorded by the ancient naturalists and historians.

Of still more fruitful births I will pass over a number of instances which I could adduce from JOHANNES RHODIUS, LUCAS SCHROECKIUS, CASPAR BAUHIN, JOHANNES HELVIGIUS, BIANCHI, and others, and finish with one case more, recorded by PETRUS BORELLI in his Second Century of Observations, published at Paris in the year 1656; a collection indeed filled with many wonderful stories, though by a man of equal integrity and ingenuity: he tells us, that in the year 1650, just five years before, the lady of the then present Lord DARRE produced at one birth eight perfect children, which he owns was a very unusual event in that country.

I think it totally unnecessary to pursue this enquiry farther; but must observe, that the present is the only case I have found, where the children were all females; that the males have in all the other cases been at least equal, and generally the most numerous; that in many of them, at least a part was dead born; and that most commonly the rest died in a short time. It is thence clear, that those numerous births are certainly unfavourable to population, as very few indeed of those children can be carried to near the full term of pregnancy, and fewer still to that degree of strength that admits of their being reared, where more than two are born at one time.

As from Mr. JOHN HUNTER's very curious Experiments and Observations, read lately to this Society, on the Procreation of Swine, we are led to believe, that a certain determined number of ova, capable of receiving male impregnation, are originally formed in each ovarium; and which number, when exhausted, the female constitution has no power to renew; if this be the
true

true account of the oeconomy of nature in this particular, which has every appearance of probability, those numerous births must occasion a very fruitless profusion and waste of the human race, and become every way detrimental to its increase.

From the united testimony of all the foregoing cases, it is undeniably clear, that the females of the human species, though most commonly uniparous, are, in certain circumstances to us unknown, every now and then capable of very far exceeding their usual number; and I must again repeat, that it does not appear that we can set any bounds to the powers of nature in that respect; or pretend, as some have done, with certainty to say, what may be the utmost limits of human fertility.



XXXV. *Chloranthus*, a new Genus of Plants, described by Olof Swartz, M. D. Communicated by Sir Joseph Banks, Bart. P. R. S.

Read June 21, 1787.

AMONG the numberless vegetable productions that have appeared in the Royal Garden at Kew, is the present. It is already long since this curious plant has been introduced there as a native of China, where, we are told, the same is cultivated in the Chinese gardens, though it seems not to have any qualities either palatable or odoriferous, nor a beautiful appearance.

At the first sight of the plant, there is some likeness of *Viscum* or *Loranthus*; and considering the inflorescentia, and the insertion of the antheræ, we find no less analogy; though on a nearer examination it is greatly different, and of a very intricate construction.

The pains I have taken to enucleate the family relation of this hitherto unknown vegetable, have induced me, for the sake of its singularity, to present it as a new genus, of which I think the following natural character may be the most proper:

Calyx nullus; sed

Squama ovata, acuta, concava, cui germen insidens.

Corolla monopetala, dimidiata, vel

Petalum unicum, subrotundum, trilobum, convexum, lateri exteriori germinis insertum, staminiiferum, deciduum.

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F f f

Lobus

Lobus intermedius cæteris major.

Stam. Filamenta nulla.

Antheræ quatuor, marginibus petali longitudinaliter accretæ, bivalves.

Pist. Germen oblongum, vel obovatum difforme, squama fere tectum, antice prominens, petaligerum.

Stylus obliquus, crassus, brevissimus, angulatus.

Stigmata tria, minutissima, erecta.

Per. Bacca oblonga, monosperma.

Semen oblongum.

From this generical description the essential character is formed :

Calyx nullus.

Cbr. Petalum trilobum lateri germinis insidens.

Antheræ petalo accretæ.

Bacca monosperma.

This new genus is to be placed in *Tetrandria Monogynia* with *flores incompleti, superi*, next after *Acæna*; and amongst the *Ordines naturales* I think it would best take its place in the XLVIII. 2. next after *Viscum*.

To distinguish this species from others, that may be discovered of the same genus, I have adopted the *nomen triviale* of

Chloranthus inconspicuus :

Its specific description is as follows :

Planta herbacea.

Caules plures ex radice, semipedales, patentes, suberecti, ramosiusculi, teretes, glabri.

Rami oppositi, patentes, teretes, striati, glabri.

Folia petiolata, decussata, opposita, lanceolato-ovata, margine ferrata, nervosa, venosa, subsucculenta, glaberrima, pallide viridia.

Petiolis

Petioli breviusculi, superne canaliculati, glabri.

Stipulae interpetiolares, utrinque denticulis duobus minutis, membranaceæ, persistentes.

Flores paniculati.

Panacula terminalis, erecta, simplex.

Racemi (vel *spicae*) oppositi, decussati, erectiusculi, subfastigiati.

Flores oppositi, decussati, sessiles, solitarii, minuti, magnitudine capitis aciculæ, subsucculenti, ex albido lutei.

Pollen flavum.

Stigmata albida.

Bacca nigra, magnitudine piperis.

I have not been able to find any description or figure answering to this plant in the works of the East-Indian naturalists. I have only met with one Chinese drawing, in the library of Sir JOSEPH BANKS, Bart. P. R. S. among some others of their garden plants, that seems to represent the present.

It is said to be called Chu-Lan by the Chinese; but it ought not to be confounded with the Tsjiulang or Camunium Chinense of Rumphius (Herb. Amboin. l. VII. cap. xv. and Auctuarii ejusd. cap. XLVII.), the description of which seems to correspond in some parts with the Chloranthus: the first figure, however, on the eighteenth plate shews the plant of RUMPHIUS to be the Vitex pinnata of LINNÆUS.

EXPLANATION OF THE PLATE. (Tab. XIV.)

The plant in natural size.

a. A part of a racemus with flowers, magnified.

b. A flower magnified, separated from the racemus, containing the pistillum, with the petalum inserted on the side of the germen.

c. A petalum separately, with its four antheræ inserted on the margins of the lobes.

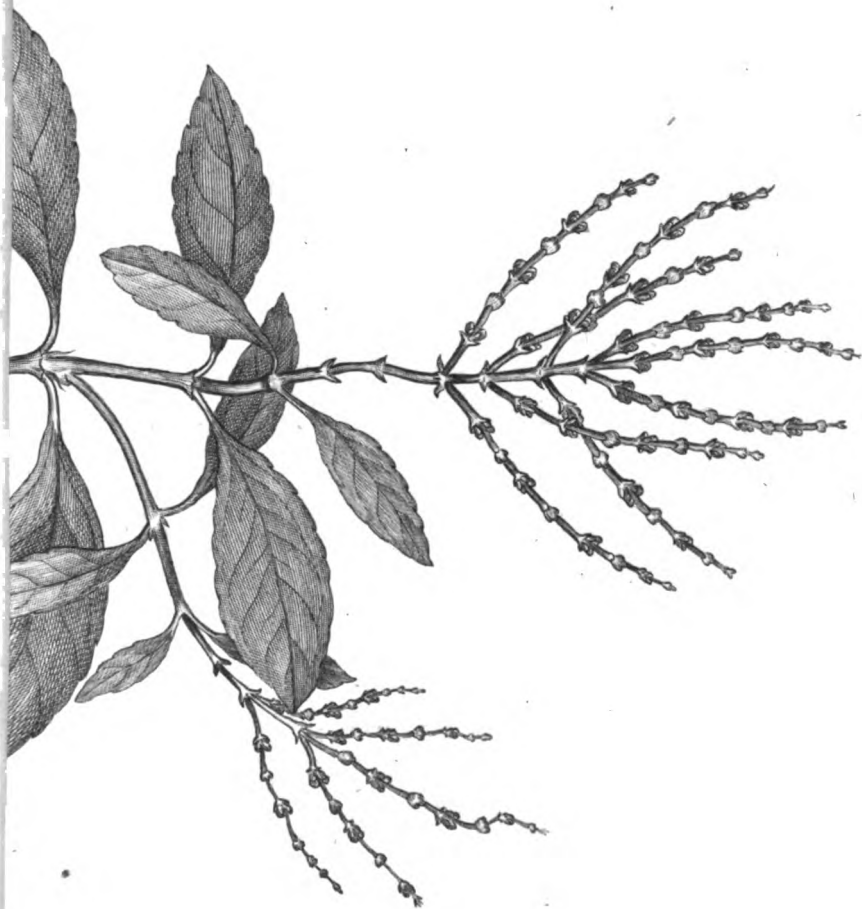
d. The germen without the corolla.

e. The germen when in flower, cut transversely, shewing the rudiment of the seed.

f. The bacca, cut transversely, with one room and seed, of natural size.

g. The form of the seed.





XXXVI. *On the Precession of the Equinoxes.* By the Rev.
Samuel Vince, M. A. F. R. S.

Read June 21, 1787.

1. **T**HE true cause of the precession of the equinoctial points was first assigned by Sir ISAAC NEWTON; but it is confessed, that he has fallen into an error in his investigation of the effect. Without, however, entering into any enquiry relative to the circumstances in which he has erred, I propose to shew how we may obtain a true solution from his own principles, by means of which alone the whole calculation may be rendered extremely simple and evident: and although very satisfactory solutions have been already given, yet the importance of the problem will sufficiently apologise for offering any thing further upon the subject that may at all tend to elucidate it.

2. Let S (Tab. XV. fig. 1.) be the sun, ABDC the earth, T its center, EQ the equator, P, *p*, the poles; draw CTB perpendicular to SAD, and join SE, which produce to meet CB in K. Call the radius ET unity, and let the force of the sun on a particle at T be $\frac{1}{ST^2}$, then the force on a particle at E = $\frac{1}{SE^2}$; hence, if we resolve this latter force into two others, one in the direction ET, and the other in the direction parallel to TS, we have $SE : ST :: \frac{1}{SE^2} : \text{the force in the direction parallel to}$

to $TS = \frac{ST}{SE^3} = \frac{ST}{ST - EK^3} = \frac{1}{ST^2} + \frac{3EK}{ST^3}$, omitting the other terms of the series on account of their smallness. Hence the force with which a particle at E is drawn *from* CB is equal to $\frac{3EK}{ST^3}$; consequently the effect of this force in a direction perpendicular to ET will be $\frac{3EK \times KT}{ST^3}$; hence this force : the force of the sun on a particle at T :: $\frac{3EK \times KT}{ST^3} : \frac{1}{ST^2} :: 3EK \times KT : ST$. Now if P = the periodic time of the earth, p = the periodic time of a body revolving at the earth's surface; then the force of the earth to the sun : the force of the body to the earth, or the force of gravity, :: $\frac{ST}{P^2} : \frac{1}{p^2}$; and hence the force of the sun on a particle at E perpendicular to ET : the force of gravity :: $\frac{3EK \times KT \times p^3}{P^2} : 1$.

3. Let v be the center of gyration, and put M = the quantity of matter in the earth : then the effect of the inertia of M placed at v , to oppose the communication of motion, is the same as the effect of the inertia of the earth; and hence, by the property of that center, $ET^2 : Tv^2 (= \frac{2}{3} ET^2) :: M : \frac{2}{3} M$, which is the quantity of matter to be placed at E to have the same effect.

4. Put m = the excess of the quantity of matter in the earth above that of its inscribed sphere. Now by Sir Isaac NEWTON's two first lemmas, it appears, that the action of the sun upon the shell m of matter, to generate an angular velocity about an axis perpendicular to CABD, is just the same as it would be to generate an angular velocity in a quantity of matter equal to $\frac{2}{3} m$ placed at E. Let us therefore suppose the sun's attraction, perpendicular to ET, to be exerted upon a
quantity

quantity of matter at E equal to $\frac{1}{3}m$, and at the same time to have a quantity of matter to move equal to $\frac{1}{3}M$, and then from this and art. 3. it appears, that the effect will be the same as the accelerative force of the sun to turn about the earth. Hence that accelerative force is, from art. 2. equal to $\frac{3EK \times KT \times p^2 \times \frac{1}{3}m}{\frac{1}{3}M \times P^2} = \frac{3EK \times KT \times p^2 \times m}{2M \times P^2}$, gravity being unity. Now, if $TE : TP :: 1 : 1 - r$, then $M : M - m :: 1 : 1 - 2r$, therefore, $M : m :: 1 : 2r$, hence $\frac{m}{2M} = r$, consequently the accelerative force = $\frac{3EK \times KT \times p^2 \times r}{P^2}$.

5. Let \dot{z} = the arc described by a point of the equator about its axis in an indefinitely small given time, which may therefore represent its velocity; and let $a\dot{z}$ represent the arc described in the same time by a body revolving about the earth at its surface; then $\frac{a^2\dot{z}^2}{2}$ = the sagitta of the arc described by the body in the same time, and consequently $a^2\dot{z}^2$ = the velocity generated by gravity whilst a point of the equator describes \dot{z} . Hence, by art. 4. we have $1 : \frac{3EK \times KT \times p^2 \times r}{P^2} :: a^2\dot{z}^2 : \frac{3EK \times KT \times p^2 \times r \times a^2\dot{z}^2}{P^2}$ the velocity of the point E of the equator generated by the action of the sun, whilst the equator describes \dot{z} about its axis; consequently the ratio of these velocities is as $\frac{3EK \times KT \times p^2 \times a^2\dot{z}^2}{P^2} : 1$.

6. Let \dot{y} be an arc described by the sun in the ecliptic to a radius equal to unity, whilst a point of the equator describes \dot{z} about its axis; then (as ap = the time of the earth's rotation, and the arcs described in equal times to equal radii are inversely as the periodic times) $\frac{1}{P} : \frac{1}{ap} :: \dot{y} : \dot{z} = \frac{P\dot{y}}{ap}$; hence, if v and w be put for

for the sine and cosine of the sun's declination, the ratio of the velocities in the last article becomes $\frac{3apr\omega y}{P} : 1$.

7. Hence if TSL (fig. 2.) be the ecliptic to the radius unity, P the plane of the sun, SER the equator, PE the sun's declination, and we take $Ec : cd$ (cd being perpendicular to Ec) :: $1 : \frac{3apr\omega y}{P}$, and through d , E, describe the great circle TEM, then will ST be the precession of the equinox during the time the sun describes y in the ecliptic. Now Ed (or Ec , as the angle at E is indefinitely small) : dc :: rad. = 1 : sine angle $E = \frac{3apr\omega y}{P}$; hence (if SV be drawn perpendicular to TE) $1 : \text{sine SE} :: \frac{3apr\omega y}{P} : SV = \frac{3apr\omega \times \text{fin. SE} \times y}{P}$; therefore, $\text{fin. STV or ESP} : 1 :: SV : ST = \frac{3apr\omega \times \text{fin. SE} \times y}{P \times \text{fin. ESP}}$.

8. Now $\frac{v}{\text{fin. ESP}} = \text{fin. SP}$, and $w = \frac{\text{cof. SP}}{\text{cof. ES}}$, hence $\frac{vw}{\text{fin. ESP}} = \frac{\text{fin. SP} \times \text{cof. SP}}{\text{cof. ES}}$; but $\frac{\text{cof. ESP}}{\tan. ES \times \cot. SP} = 1$, hence $\frac{vw}{\text{fin. ESP}} = \frac{\text{fin. SP} \times \text{cof. SP} \times \text{cof. ESP}}{\text{cof. ES} \times \tan. ES \times \cot. SP} = \frac{\text{fin. SP}^2 \times \text{cof. ESP}}{\text{fin. ES}}$; consequently,

$$ST = \frac{3apr \times \text{fin. SP}^2 \times \text{cof. ESP} \times y}{P} = (\text{if } x = \text{fin. SP})$$

$\frac{3apr \times \text{cof. ESP} \times x^2 y}{P \times \sqrt{1-x^2}}$, whose fluent, when $x = 1$, is $\frac{3apr \times \text{cof. ESP} \times y}{2P}$

(y being now = to a quadrant) the arc of precession whilst the sun describes 90° of the ecliptic; and to find the degrees say,

$$4y : 360^\circ :: \frac{3apr \times \text{cof. ESP} \times y}{2P} : 360^\circ \times \frac{3apr \times \text{cof. ESP}}{8P}, \text{ consequently}$$

$$\text{the precession in a year} = 360^\circ \times \frac{3apr \times \text{cof. ESP}}{2P} = 21'' 6'''. \text{ This}$$

would be the precession of the equinox arising from the attraction of the sun, if the earth were of an uniform density, and the ratio

of

of the diameters as 229 : 230 ; but if the greatest nutation of the earth's axis be rightly ascertained, the precession is only about $14''\frac{2}{3}$; which difference between the theory and what is deduced from observation must arise either from the fluidity of the earth's surface, an increase of density towards the center, or the ratio of the diameters being different from what is here assumed, or probably from all the causes conjointly. But as the best observations must be liable to some small degree of inaccuracy, and an error of one or two seconds in the nutation will, in this case, make a very considerable alteration in the conclusion, the estimation of the precession arising from the action of the sun seems to be subject to a very considerable degree of uncertainty.



XXXVII. *Abstract of a Register of the Barometer, Thermometer, and Rain at Lyndon in Rutland, in 1786. By Thomas Barker, Esq. Also of the Rain at South-Lambeth, in Surrey; and at Selbourn and Fyfield, Hampshire. Communicated by Thomas White, Esq. F.R.S.*

Read June 21, 1786.

		Barometer.			Thermometer.						Rain.			
					In the House.			Abroad.			Lyndon	S. Lambeth.	Selbourn.	Fyfield.
		Highest	Lowest	Mean.	High.	Low.	Mean	High	Low.	Mean				
		Inches.	Inches.	Inches.	°	°	°	°	°	°	Inch.	Inch.	Inch.	Inch.
Jan.	Morn.	29,87	28,33	29,18	49	25	38	48½	11½	34	3,467	2,48	6,58	4,63
	Aftern.				50	25	39	53	19	39				
Feb.	Morn.	29,96	28,84	29,50	48	32½	40	45½	24	34	0,665	1,08	1,27	4,75
	Aftern.				46½	33	41	49	27½	39½				
Mar.	Morn.	29,78	28,76	29,29	46	28	37	46	18	30	0,832	1,11	1,53	1,62
	Aftern.				47½	29	38	50½	22½	39				
Apr.	Morn.	29,96	29,02	29,46	55½	38½	47	52	29	41	1,252	1,22	1,63	1,41
	Aftern.				57	40	48	68	37	51				
May	Morn.	29,90	28,75	29,46	63½	45	53	59½	32	48½	2,383	0,97	2,16	2,73
	Aftern.				65½	46½	55	73	49	59½				
June	Morn.	29,90	29,32	29,53	66	58½	62	65	49	56½	1,583	2,24	1,05	1,51
	Aftern.				69	59	64	80½	60	68				
July	Morn.	30,00	29,05	29,56	65	56½	61½	62½	50	56	1,799	0,86	1,81	1,42
	Aftern.				66½	58½	63	74	51	66				
Aug.	Morn.	29,84	20,01	29,43	67	57	61	65	48	55	2,632	1,19	4,00	3,57
	Aftern.				68	58½	62	75½	57	65½				
Sept.	Morn.	29,98	28,40	29,35	61	49	55	58	38	47	2,840	8,22	4,50	1,62
	Aftern.				61	49	56½	67	47	57				
Oct.	Morn.	30,00	28,29	29,55	54½	45	49	50	31	41	4,762		5,04	4,18
	Aftern.				55½	45½	50	62½	42	49				
Nov.	Morn.	29,81	28,55	29,36	45	36½	41	45	27	35	2,938	3,06	4,38	1,22
	Aftern.				45½	36½	41½	47½	30	39				
Dec.	Morn.	30,05	28,44	29,15	46	32	39½	45½	Thermom. broken.		2,136		5,62	4,13
	Aftern.				46	33	40½	46						
Inches											27,289	22,43	39,57	29,60

The

The frosts at the end of 1785 and beginning of 1786 were severe, but not long, with large snows the end of December and middle of January. The intervals were wet in January and windy, but mild in February. At this time some farmers sowed a little barley, which, after lying a great while in the ground during the following frost, came up well at last, was forward, and prospered. The end of February and beginning of March was the longest frost this winter, being a full fortnight, and the wind being strong from the east, and no snow at first, and when things were getting forward, it did more hurt than all the frosts this winter; and the winds continued much N.E. and frequently frosty the rest of the month. The seed time was good, but rather backward, and the weather in general dry in February, March, and April, the wind often N.E. and the season backward, yet not so many frosty nights as in some late springs, and the latter half of April mild and growing. After some frosty mornings the beginning of May it was in general a growing month, and a fine rain before the middle made plenty of grass; and the latter end of May, and most part of June and July, being fine and moderately hot, I think, I never knew so much hay so well got in any year before, which was of great service, as there was scarce any old left. The first sown turneps stood very well; but the dryness of the season hindered the latter sown from coming up well till August, and many were small; and the season was drier, and the ground more burnt, in the north of England than here.

The last day of July began a cool, showery season, which much improved the grass and turneps, but hindered the beginning of harvest, which, however, was afterward well gotten. The crop of barley this year was great, the wheat good, oats indifferent: but a great part of the beans never came up well;

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whether

whether because the seed was ill gotten last year, or because a frosty March after they were sown spoiled them. I think I scarce ever knew more north winds in summer, or more east winds in October or November, than this year. From August to December there was a great deal of rain by fits, particularly the middle of August, about Michaelmas, the 6th to the 12th of October, the third week in November, and first half of December; yet with intervals of fair fine weather between. It was much windy, and some great storms on September 14, October 8, and December 14. The N.E. winds in October and November brought on cold weather early; and the accounts from the northern countries complained of a severe beginning of winter, almost shutting up the Baltic before the usual time; but the rest of the winter proved very different. After the rains in the former part of December, the year ended frosty, and to Christmas as sharp as any this winter; but no way remarkable, and with little of either rain or snow.

Since I saw that corona about the Moon, mentioned in Philosophical Transactions, Vol. LXXIII. p. 245. I have some few times seen a very faint appearance of it, and made the following remarks about it. The common bright circle round the moon, bounded by a yellowish red, is of much larger diameter, more diffused, and fainter, when the air is warm and the clouds misty; but no corona then appears. The time to expect it is in a frost, or when inclined to it, the clouds better defined and white; that first circle is then much less in diameter and brighter. It was remarkably so when the corona appeared November 17, 1782: and it has always been so in some measure, whenever there has been any tendency to it since.



XXXVIII. *Observations on the Structure and Oeconomy of Whales.*

By John Hunter, Esq. F. R. S. ; communicated by Sir Joseph Banks, Bart. P. R. S.

Read June 28, 1787.

THE animals which inhabit the sea are much less known to us than those found upon land; and the œconomy of those with which we are best acquainted is much less understood: we are, therefore, too often obliged to reason from analogy where information fails; which must probably ever continue to be the case, from our unsuitness to pursue our researches in the unfathomable waters.

This unsuitness does not arise from that part of our œconomy on which life and its functions depend; for the tribe of animals which is to be the subject of this Paper, has, in that respect, the same œconomy as man, but from a difference in the mechanism by which our progressive motion is produced.

The anatomy of the larger marine animals, when they are procured in a proper state, can be as well ascertained as that of any others; dead structure being readily investigated. But even such opportunities too seldom occur, because those animals are only to be found in distant seas, which no one explores in pursuit of natural history; neither can they be brought to us alive from thence, which prevents our receiving their bodies in a state

state fit for dissection. As they cannot live in air, we are unable to procure them alive.

Some of these aquatic animals yielding substances which have become articles of traffic, and in quantity sufficient to render them valuable as objects of profit, are sought after for that purpose; but gain being the primary view, the researches of the Naturalist are only considered as secondary points, if considered at all. At the best, our opportunities of examining such animals do not often occur till the parts are in such a state as to defeat the purposes of accurate enquiry, and even these occasions are so rare as to prevent our being able to supply, by a second dissection, what was deficient in a first. The parts of such animals being formed on so large a scale, is another cause which prevents any great degree of accuracy in their examination; more especially when it is considered, how very inconvenient for accurate dissections are barges, open fields, and such places as are fit to receive animals, or parts, of such vast bulk.

As the opportunities of ascertaining the anatomical structure of large marine animals are generally accidental, I have availed myself, as much as possible, of all that have occurred; and, anxious to get more extensive information, engaged a Surgeon, at a considerable expence, to make a voyage to Greenland, in one of the ships employed in the whale fishery, and furnished him with such necessaries as I thought might be requisite for examining and preserving the more interesting parts, and with instructions for making general observations; but the only return I received for this expence was a piece of whale's skin, with some small animals sticking upon it. From the opportunities which I have had of examining different animals of this order, I have gained a tolerably accurate idea of the

anatomical structure of some genera, and such a knowledge of the structure of particular parts of some others, as to enable me to ascertain the principles of their œconomy.

Those which I have had opportunities of examining were the following :

Of the *Delphinus Phocæna*, or Porpoise, I have had several, both male and female.

Of the *Grampus* I have had two ; one of them (Tab. XVI.) twenty-four feet long, the belly of a white colour, which terminated at once, the sides and back being black ; the other (Tab. XVII.) about eighteen feet long, the belly white, but less so than in the former, and shaded off into the dark colour of the back.

Of the *Delphinus Delphis*, or Bottle-nose Whale (Tab. XVIII.) I had one sent to me by Mr. JENNER, Surgeon, at Berkeley. It was about eleven feet long. I have also had one twenty-one feet long, resembling this last in the shape of the head, but of a different genus, having only two teeth in the lower jaw (Tab. XIX.) ; the belly was white, shaded off into the dark colour of the back. This species is described by DALE, in his *Antiquities of Harwich*. The one which I examined must have been young ; for I have a skull of the same kind, nearly three times as large, which must have belonged to an animal thirty or forty feet long.

Of the *Balæna rostrata* of FABRICIUS, I had one, seventeen feet long (Tab. XX.).

The *Balæna Mysticetus*, or large Whalebone Whale, the *Physeter Macrocephalus*, or Spermaceti Whale, and the *Monodon Monoceros*, or Narwhale, have also fallen under my inspection. Some of these I have had opportunities of examining with accuracy ; while others I have only examined in
part,

part, the animals having been too long kept before I procured them, to admit of more than a very superficial inspection.

From these circumstances it will be readily supposed, that an accurate description of all the different species is not to be expected; but having acquired a general knowledge of the whole tribe, from the different species which have come under my examination, I have been enabled to form a tolerable idea, even of parts which I have only had the opportunity of seeing in a very cursory way.

General observation would lead us to believe, that the whole of this tribe constitutes one order of animals, which Naturalists have subdivided into genera and species; but a deficiency in the knowledge of their œconomy has prevented them from making these divisions with sufficient accuracy; and this is not surprising, since the genera and species are still in some measure undetermined even in animals with which we are better acquainted.

The animals of this order are in size the largest known, and probably, therefore, the fewest in number of all that live in water. Size, I believe, in those animals who feed upon others, is in an inverse proportion to the number of the smaller; but, I believe, this tribe varies more in that respect than any we know, viewing it from the Whalebone Whale, which is seventy or eighty feet long, to the Porpoise that is five or six: however, if they differ as much among themselves as the Salmon does from the Sprat, there is not that comparative difference in size that would at first appear. The Whalebone Whale is, I believe, the largest; the Spermaceti Whale the next in size (the one which I examined, although not full grown, was about sixty feet long); the Grampus, which is an extensive genus, is probably

probably from twenty to fifty feet long; under this denomination there is a number of species.

From my want of knowledge of the different genera of this tribe of animals, an incorrectness in the application of the anatomical account to the proper genus may be the consequence; for when they are of a certain size, they are brought to us as Porpoises; when larger, they are called Grampus, or Fin-fish. A tolerably correct anatomical description of each species, with an accurate Drawing of the external form, would lead us to a knowledge of the different genera, and the species in each; and, in order to forward so useful a work, I propose, at some future period, to lay before the Society descriptions and drawings of those which have come under my own observation.

From some circumstances in their digestive organs we should be led to suppose, that they were nearly allied to each other; and that there was not the same variety, in this respect, as in land animals.

In the description of this order of animals, I shall always keep in view their analogy to land animals, and to such as occasionally inhabit the water, as white Bears, Seals, Manatees, &c. with the differences that occur. This mode of referring them to other animals, better known, will assist the mind in understanding the present subject. It is not, however, intended in this Paper to give a particular account of the structure of all the animals of this order, which I have had an opportunity of examining: I propose, at present, chiefly to confine myself to general principles, giving the great outlines as far as I am acquainted with them, minuteness being only necessary in the investigation of particular parts.

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In my account I shall pay some attention to the relations of men who have given facts without knowing their causes, whenever I find that such facts can be explained upon true principles of the animal œconomy, but no further.

This order of animals has nothing peculiar to fish, except living in the same element, and being endowed with the same powers of progressive motion as those fish that are intended to move with a considerable velocity: for I believe, that all that come to the surface of the water (which this order of animals must do) have considerable progressive motion; and this reasoning we may apply to birds; for those which soar very high have the greatest progressive motion.

Although inhabitants of the waters, they belong to the same class as quadrupeds, breathing air, being furnished with lungs, and all the other parts peculiar to the œconomy of that class, and having warm blood; for we may make this general remark, that in the different classes of animals there is never any mixture of those parts which are essential to life, nor in their different modes of sensation.

I shall divide what is called the œconomy of an animal, first, into those parts and actions which respect its internal functions, and on which life immediately depends, as growth, waste, repair, shifting or changing of parts, &c. the organs of respiration and secretion, in which we may include the powers of propagating the species.

Secondly, into those parts and actions which respect external objects, and which are variously constructed, according to the kind of matter with which they are to be connected, whence they vary, more than those of the first division. These are the parts for progressive motion, the organs of sense
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and the organs of digestion ; all which either act, or are acted upon, by external matter.

This variation from external causes in many instances influences the shape of the whole, or particular parts, even giving a peculiar form to some which belong to the first order of actions, as the heart, which in this tribe, in the Seal, Otter, &c. is flattened, because the chest is flattened for the purpose of swimming. The contents of the abdomen are not only adapted to the external form ; but their direction in the cavity is, in some instances, regulated by it. The anterior extremity, or fin, although formed of distinct parts, in some degree similar to the anterior extremities of some quadrupeds, being composed of similar bones placed nearly in the same manner, yet are so formed and arranged as to fit them for progressive motion in the water only.

The external form of this order of animals is such as fits them for dividing the water in progressive motion, and gives them power to produce that motion in the same manner as those fish which move with a considerable velocity. On account of their inhabiting the water, their external form is more uniform than in animals of the same class which live upon land, the surface of the earth on which the progressive motion of the quadruped is to be performed being various and irregular, while the water is always the same.

The form of the head or anterior part of this order of animals is commonly a cone, or an inclined plane, except in the *Spermæceti* Whale, in which it terminates in a blunt surface. This form of head increases the surface of contact to the same volume of water which it removes, lessens the pressure, and is better calculated to bear the resistance of the water through which the animal is to pass ; probably, on this account, the head

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is larger than in quadrupeds, having more the proportion observed in fish, and swelling out laterally at the articulation of the lower jaw: this may probably be for the better catching their prey, as they have no motion of the head on the body; and this distance between the articulations of the jaw is somewhat similar to the Swallow, Goat-sucker, Bat, &c. which may also be accounted for, from their catching their food in the same manner as fish; and this is rendered still more probable, since the form of the mouth varies according as they have or have not teeth. There is, however, in the Whale tribe more variety in the form of the head than of any other part, as in the Whalebone, Bottlenose, and Spermaceti Whales; though in this last it appears to owe its shape, in some sort, to the vast quantity of spermaceti lodged there, and not to be formed merely for the catching of its prey. From the mode of their progressive motion, they have not the connection between the head and body, that is called the neck, as that would have produced an inequality inconvenient to progressive motion.

The body behind the fins or shoulders diminishes gradually to the spreading of the tail; but the part beyond the opening of the anus is to be considered as tail, although to appearance it is a continuation of the body. The body itself is flattened laterally; and, I believe, the back is much sharper than the belly.

The projecting part, or tail, contains the power that produces progressive motion, and moves the broad termination, the motion of which is similar to that of an oar in sculling a boat; it supersedes the necessity of posterior extremities, and allows of the proper shape for swimming; that the form may be preserved as much as possible, we find that all the projecting

parts, found in land animals of the same class, are either intirely wanting, as the external ear; are placed internally; as the testicles; or are spread along under the skin, as the udder.

The tail is flattened horizontally, which is contrary to that of fish, this position of tail giving the direction to the animal in the progressive motion of the body. I shall not pursue this circumstance further than to apply it to those purposes in the animal oeconomy, for which this particular direction is intended.

The two lateral fins, which are analogous to the anterior extremities in the quadruped, are commonly small, varying however in size, and seem to serve as a kind of oars.

To ascertain the use of the *fin* on the back is probably not so easy, as the large Whalebone and Spermaceti Whales have it not; one should otherwise conceive it intended to preserve the animal from turning.

I believe, like most animals, they are of a lighter colour on their belly than on their back: in some they are intirely white on the belly; and this white colour begins by a regular determined line, as in the Grampus, Piked Whale, &c.: in others, the white on the belly is gradually shaded into the dark colour of the back, as in the Porpoise. I have been informed, that some of them are pied upwards and downwards, or have the divisions of colour in a contrary direction

The element in which they live renders certain parts which are of importance in other animals useless in them, gives to some parts a different action, and renders others of less account.

The puncta lachrymalia with the appendages, as the sac and duct, are in them unnecessary; and the secretion from the lachrymal gland is not water, but mucus, as it also is in the Turtle;
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and we may suppose only in small quantity, the gland itself being small.

The urinary bladder is smaller than in quadrupeds; and indeed there is not any apparent reason why Whales should have one at all.

The tongue is flat, and but little projecting, as they neither have voice, nor require much action of this part, in applying the food between the teeth for the purpose of mastication, or deglutition, being nearly similar to fish in this respect, as well as in their progressive motion.

In some particulars they differ as much from one another as any two genera of quadrupeds I am acquainted with.

The larynx, size of trachea, and number of ribs, differ exceedingly. The cæcum is only found in some of them. The teeth in some are wanting. The blow-holes are two in number in many, in others only one. The whalebone and spermaceti are peculiar to particular genera: all which constitute great variations. In other respects we find an uniformity, which would appear to be independent of their living and moving only in the water, as in the stomach, liver, parts of generation of both sexes, and in the kidneys: in these last however, I believe, it depends in some degree upon their situation, although it is extended to other animals, the cause of which I do not understand.

All animals have, I believe, a smell peculiar to themselves: how far this is connected with the other distinctions, I do not know, our organs not being able to distinguish with sufficient accuracy.

The smell of animals of this tribe is the same with that of the Seal, but not so strong, a kind of sour smell, which the

Seal has while alive; the oil has the same smell with that of the Salmon, Herring, Sprat, &c.

The observations respecting the weight of the flesh of animals that swim, which I published in my observations on the oeconomy of certain parts of animals, are applicable to these also; for the flesh in this tribe is rather heavier than beef; two portions of muscle of the same shape, one from the psoas muscle of the Whale, the other of an ox, when weighed in air, were both exactly 502 grains; but, weighed in water, the portion of the Whale was four grains heavier than the other. It is probable, therefore, that the necessary equilibrium between the water and the animal is produced by the oil, in addition to which the principal action of the tail is such as tends either to raise them, or keep them suspended in the water, according to the degree of force with which it acts.

From the tail being horizontal, the motion of the animal, when impelled by it, is up and down: two advantages are gained by this, it gives the necessary opportunity of breathing, and elevates them in the water; for every motion of the tail tends, as I said before, to raise the animal: and that this may be effected, the greatest motion of the tail is downwards, those muscles being very large, making two ridges in the abdomen; this motion of the tail raises the anterior extremity, which always tends to keep the body suspended in the water.

Of the Bones.

The bones alone, in many animals, when properly united into what is called the skeleton, give the general shape and character of the animal. Thus a quadruped is distinguished from a bird, and even one quadruped from another, it only requiring

requiring a skin to be thrown over the skeleton to make the species known; but this is not so decidedly the case with this order of animals, for the skeleton in them does not give us the true shape. An immense head, a small neck, few ribs, and in many a short sternum, and no pelvis, with a long spine, terminating in a point, require more than a skin being laid over them to give the regular and characteristic form of the animal.

The bones of the anterior extremity give no idea of the shape of a fin, the form of which depends wholly upon its covering. The different parts of the skeleton, are so inclosed, and the spaces between the projecting parts are so filled up, as to be altogether concealed, giving the animal externally an uniform and elegant form, resembling an insect enveloped in its chrysalis coat.

The bones of the head are in general so large, as to render the cavity which contains the brain but a small part of the whole; while, in the human species, and in birds, this cavity constitutes the principal bulk of the head. This is, perhaps, most remarkable in the *Spermaceti* Whale; for on a general view of the bones of the head, it is impossible to determine where the cavity of the skull lies, till led to it by the foramen magnum occipitale. The same remark is applicable to the large *Whalebone* and *Bottle-nose* Whale; but in the *Porpoise*, where the brain is larger in proportion to the size of the animal, the skull makes the principal part of the head.

Some of the bones in one genus differ from those of another. The lower jaw is an instance of this. In the *Spermaceti* and *Bottle-nose* Whales, the *Grampus*, and the *Porpoise*, the lower jaws, especially at the posterior ends, resemble each other;

other; but in both the large and small Whalebone Whales, the shape differs considerably. The number of some particular bones varies likewise very much.

The Piked Whale has seven vertebræ in the neck, twelve which may be reckoned to the back, and twenty-seven to the tail, making forty-six in the whole.

In the porpoise there are five cervical vertebræ, and one common to the neck and back, fourteen proper to the back, and 30 to the tail, making in the whole fifty-one.

The small Bottle-nose Whale, caught near Berkeley, in the number of cervical vertebræ resembled the Porpoise; it had seventeen in the back, and thirty-seven in the tail, in all sixty.

In the Porpoise, four of the vertebræ of the neck are anchylosed; and in every animal of this order, which I have examined, the atlas is by much the thickest, and seems to be made up of two joined together, for the second cervical nerve passes through a foramen in this vertebra. There is no articulation for rotatory motion between the first and second vertebræ of the neck.

The small Bottle-nose Whale had eighteen ribs on each side, the Porpoise sixteen. The ends of the ribs that have two articulations, in the whole of this tribe, I believe, are articulated with the body of the vertebræ above, and with the transverse processes below, by the angles; so that there is one vertebra common to the neck and back. In the large Whalebone Whale the first rib is bifurcated, and consequently articulated to two vertebræ.

The sternum is very flat in the Piked Whale; it is only one very short bone; and in the Porpoise it is a good deal longer. In the small Bottle-nose it is composed of three

bones, and is of some length. In the Piked Whale the first rib, and in the Porpoise the three first, are articulated with the sternum.

As a contraction, corresponding to the neck in quadrupeds, would have been improper in this order of animals, the vertebræ of the neck are thin, to make the distance between the head and shoulders as short as possible, and in the small Bottle-nose Whale are only six in number.

The structure of the bones is similar to that of the bones of quadrupeds; they are composed of an animal substance, and an earth that is not animal: these seem only to be mechanically mixed, or rather the earth thrown into the interstices of the animal part. In the bones of fishes this does not seem to be the case, the earth in many fish being so united with the animal part, as to render the whole transparent, which is not the case when the animal part is removed by steeping the bone in caustic alkali: nor is the animal part so transparent when deprived of the earth. The bones are less compact than those of quadrupeds that are similar to them.

Their form somewhat resembles what takes place in the quadruped, at least in those whose uses are similar, as the vertebræ, ribs, and bones of the anterior extremities have their articulations in part alike, although not in all of them. The articulation of the lower jaw, of the carpus, metacarpus, and fingers, are exceptions. The articulation of the lower jaw is not by simple contact either single or double, joined by a capsular ligament, as in the quadruped; but by a very thick intermediate substance of the ligamentous kind, so interwoven that its parts move on each other, in the interstices of which is an oil. This thick matted substance may answer the same purpose as the double joint in the quadruped.

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The two fins are analogous to the anterior extremities of the quadruped, and are also somewhat similar in construction. A fin is composed of a scapula, os humeri, ulna, radius, carpus, and metacarpus, in which last may be included the fingers, because the number of bones are those which might be called fingers, although they are not separated, but included in one general covering with the metacarpus. They have nothing analogous to the thumb, and the number of bones in each is different; in the fore-finger there are five bones, in the middle and ring-finger seven, and in the little finger four. The articulation of the carpus, metacarpus, and fingers, is different from that of the quadruped, not being by capsular ligament, but by intermediate cartilages connected to each bone. These cartilages between the different bones of the fingers are of considerable length, being nearly equal to one-half of that of the bone; and this construction of the parts gives firmness, with some degree of pliability, to the whole.

As this order of animals cannot be said to have a pelvis, they of course have no os sacrum, and therefore the vertebræ are continued on to the end of the tail; but with no distinction between those of the loins and tail. But as those vertebræ alone would not have had sufficient surface to give rise to the muscles requisite to the motion of the tail, there are bones added to the fore-part of some of the first vertebræ of the tail, similar to the spinal processes on the posterior surface.

From all these observations we may infer, that the structure, formation, arrangement, and the union of the bones, which compose the forms of parts in this order of animals, are much upon the same principle as in quadrupeds.

The flesh or muscles of this order of animals is red, resembling that of most quadrupeds, perhaps more like that of the

Bull or Horse than any other animal: some of it is very firm; and about the breast and belly it is mixed with tendon.

Although the body and tail is composed of a series of bones connected together and moved as in fish, yet it has its movements produced by long muscles, with long tendons, which renders the body thicker, while the tail at its stem is smaller than that of any other swimmer, whose principal motion is the same. Why this mode of applying the moving powers should not have been used in fish, is probably not so easily answered; but in fish the muscles of the body are of nearly the same length as the vertebræ.

The depressor muscles of the tail, which are similar in situation to the psoæ, make two very large ridges on the lower part of the cavity of the belly, rising much higher than the spine, and the lower part of the aorta passes between them.

These two large muscles, instead of being inserted into two extremities as in the quadruped, go to the tail, which may be considered in this order of animals as the two posterior extremities united into one.

Their muscles, a very short time after death, lose their fibrous structure, become as uniform in texture as clay or dough, and even softer. This change is not from putrefaction, as they continue to be free from any offensive smell, and is most remarkable in the psoæ muscles, and those of the back.

Of the Construction of the Tail.

The mode in which the tail is constructed is, perhaps, as beautiful, as to the mechanism, as any part of the animal. It is wholly composed of three layers of tendinous fibres, covered by the common cutis and cuticle: two of these layers

are external, the other internal. The direction of the fibres of the external layers is the same as in the tail, forming a stratum about one-third of an inch thick; but varying, in this respect, as the tail is thicker or thinner. The middle layer is composed entirely of tendinous fibres, passing directly across, between the two external ones above described, their length being in proportion to the thickness of the tail; a structure which gives amazing strength to this part.

The substance of the tail is so firm and compact, that the vessels retain their dilated state, even when cut across; and this section consists of a large vessel surrounded by as many small ones as can come in contact with its external surface; which of these are arteries, and which veins, I do not know.

The fins are merely covered with a strong condensed adipose membrane.

Of the Fat.

The fat of this order of animals, except the spermaceti, is what we generally term oil. It does not coagulate in our atmosphere, and is probably the most fluid of animal fats; but the fat of every different order of animals has not a peculiar degree of solidity, some having it in the same state, as the Horse and Bird. What I believe approaches nearest to spermaceti, is the fat of ruminating animals, called tallow.

The fat is differently situated in different orders of animals; probably for particular purposes, at least in some we can assign a final intention. In the animals, which are the subject of the present Paper, it is found principally on the outside of the muscles, immediately under the skin, and is in considerable quantity. It is rarely to be met with in the interstices of the muscles, or in any of the cavities, such as the abdomen or about the heart.

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In animals of the same class living on land, the fat is more diffused: it is situated, more especially when old, in the interstices of muscles, even between the fasciculi of muscular fibres, and is attached to many of the viscera; but many parts are free from fat, unless when diseased, as the penis, scrotum, testicle, eyelid, liver, lungs, brain, spleen, &c.

In fish its situation is rather particular, and is most commonly in two modes; in the one, diffused through the whole body of the fish, as in the Salmon, Herring, Pilchard, Sprat, &c.; in the other, it is found in the liver only, as in all of the Ray kind, Cod, and in all those called White-fish, there being none in any other part of the body *. The fat of fish appears to be diffused through the substance of the parts which contain it, but is probably in distinct cells. In some of these fish, where it is diffused over the whole body, it is more in some parts than others, as on the belly of the Salmon, where it is in larger quantity.

The fat is differently inclosed in different orders of animals. In the quadruped, those of the Seal kind excepted, in the bird, amphibia, and in some fish, it is contained in loose cellular membrane, as if in bags, composed of smaller ones, by which means the larger admit of motion on one another, and on their connecting parts; which motion is in a greater or less degree, as is proper or useful. Where motion could answer no purpose, as in the bones, it is confined in still smaller cells. The fat is in a less degree in the soles of the feet, palms of the hands, and in the breasts of many animals. In this order of animals and the Seal kind, as far as I yet know, it is disposed of in two ways; the small quantity found in the cavities of the body,

* The Sturgeon is, however, an exception, having its fat in particular situations, and in the interstices of parts, as in other animals.

and

and interstices of parts, is in general disposed in the same way as in quadrupeds; but the external, which includes the principal part, is inclosed in a reticular membrane, apparently composed of fibres passing in all directions, which seem to confine its extent, allowing it little or no motion on itself, the whole, when distended, forming almost a solid body. This, however, is not always the case in every part of animals of this order; for under the head, or what may be rather called neck, of the Bottlenose, the fat is confined in larger cells, admitting of motion. This reticular membrane is very fine in some, and very strong and coarse in others, and even varies in different parts of the same animal. It is fine in the Porpoise, Spermaceti, and large Whalebone Whale; coarse in the Grampus and small Whalebone Whale*: in all of them it is finest on the body, becoming coarser towards the tail, which is composed of fibres without any fat: which is also the case in the covering of the fins. This reticular net-work in the Seal is very coarse; and in those which are not fat, when it collapses, it looks almost like a fine net with small meshes. This structure confines the animal to a determined shape, whereas in quadrupeds fat when in great quantity destroys all shape.

The fat differs in consistence in different animals, and in different parts of the same animal, in which its situation is various. In quadrupeds, some have the external fat softer than the internal; and that inclosed in bones is softest nearer to their extremities. Ruminating animals have that species of fat called tallow, and in their bones they have either hard fat or marrow, or fluid fat called Neat's-foot oil. In this order of animals, the internal fat is the least fluid, and is nearly of the consistence of Hog's-lard; the

* Where it is fine, it yields the largest quantity of oil, and requires the least boiling.

external is the common train oil; but the Spermaceti Whale differs from every other animal I have examined, having the two kinds of fat just mentioned, and another which is totally different, called spermaceti, of which I shall give a particular account.

What is called spermaceti is found every where in the body in small quantity, mixed with the common fat of the animal, bearing a very small proportion to the other fat. In the head it is the reverse, for there the quantity of spermaceti is large when compared to that of the oil, although they are mixed, as in the other parts of the body.

As the spermaceti is found in the largest quantity in the head, and in what would appear on a slight view to be the cavity of the skull, from a peculiarity in the shape of that bone, it has been imagined by some to be the brain.

These two kinds of fat in the head are contained in cells, or cellular membrane, in the same manner as the fat in other animals; but besides the common cells there are larger ones, or ligamentous partitions going across, the better to support the vast load of oil, of which the bulk of the head is principally made up.

There are two places in the head where this oil lies; these are situated along its upper and lower part: between them pass the nostrils, and a vast number of tendons going to the nose and different parts of the head.

The purest spermaceti is contained in the smallest and least ligamentous cells: it lies above the nostril, all along the upper part of the head, immediately under the skin; and common adipose membrane. These cells resemble those which contain the common fat in the other parts of the body nearest the skin. That which lies above the roof of the mouth, or between it
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and the nostril, is more intermixed with a ligamentous cellular membrane, and lies in chambers whose partitions are perpendicular. These chambers are smaller the nearer to the nose, becoming larger and larger towards the back part of the head, where the spermaceti is more pure.

This spermaceti, when extracted cold, has a good deal the appearance of the internal structure of a water melon, and is found in rather solid lumps.

About the nose, or anterior part of the nostril, I discovered a great many vessels, having the appearance of a plexus of veins, some as large as a finger. On examining them, I found they were loaded with the spermaceti and oil; and that some had corresponding arteries. They were most probably lymphatics; therefore I should suppose, that their contents had been absorbed from the cells of the head. We may the more readily suppose this, from finding many of the cells, or chambers, almost empty; and as we may reasonably believe that this animal had been some time out of the seas in which it could procure proper food, it had perhaps lived on the superabundance of oil.

The solid masses are what are brought home in casks for spermaceti.

I found, by boiling this substance, that I could easily extract the spermaceti and oil which floated on the top from the cellular membrane. When I skimmed off the oily part, and let it stand to cool, I found that the spermaceti crystallised, and the whole became solid; and by laying this cake upon any spongy substance, as chalk, or on a hollow body, the oil drained all off, leaving the spermaceti pure and white. These crystals were only attached to each other by edges, forming a spongy mass; and by melting this pure spermaceti, and allowing it to cry-

stallife, it was reduced in appearance to half its bulk, the crystals being smaller, and more blended, consequently less distinct.

The spermaceti mixes readily with other oils, while it is in a fluid state, but separates or crystallises whenever it is cooled to a certain degree; like two different salts being dissolved in water, one of which will crystallise with a less degree of evaporation than the other; or, if the water is warm, and fully saturated, one of the salts will crystallise sooner than the other, while the solution is cooling. I wanted to see whether spermaceti mixed equally well with the expressed oils of vegetables when warm, and likewise separated and crystallised when cold, and on trial there seemed to be no difference. When very much diluted with the oil, it is dissolved or melted by a much smaller degree of heat than when alone; and this is the reason, perhaps, that it is in a fluid state in the living body.

If the quantity of spermaceti is small in proportion to the other oil, it is, perhaps, nearly in that proportion longer in crystallising; and when it does crystallise, the crystals are much smaller than those that are formed where the proportion of spermaceti is greater. From the slowness with which the spermaceti crystallises when much diluted with its oil, from a considerable quantity being to be obtained in that way, and from its continuing for years to crystallise, one would be induced to think, that perhaps the oil itself is converted into spermaceti.

It is most likely, that if we could discover the exact form of the different crystals of oils, we should thence be able to ascertain both the different sorts of vegetable oils, expressed and essential, and the different sorts of animal oils, much better than by any other means; in the same manner as we know salts by the forms into which they shoot.

The spermaceti does not become rancid, or putrid, nearly so soon as the other animal oils; which is most probably owing to the spermaceti being for the most part in a solid state; and I should suppose, that few oils would become so soon rancid as they do, if they were always preserved in that degree of cold which rendered them solid: neither does this oil become so soon putrid as the flesh of the animal; and therefore, although the oil in the cells appeared to be putrid before boiling, it was sweet when deprived of the cellular substance. The spermaceti is rather heavier than the other oil.

In this animal then we find two sorts of oil, besides the deeper seated fat, common to all of this class; one of which crystallises with a much less degree of cold than the other, and of course requires a greater degree of heat to melt it, and forms, perhaps, the largest crystals of any expressed oil we know: yet the fluid oil of this animal will crystallise in an extreme hard frost, much sooner than most essential oils, though not so soon as the expressed oils of vegetables. Camphire, however, is an exception, since it crystallises in our warmest weather, and when melted with expressed oil of vegetables, if the oil is too much saturated for that particular degree of cold, crystallises exactly like spermaceti.

In the Ox the tallow, and what is called Neat's-foot oil, crystallise in different degrees of cold. The tallow congeals with rather less cold than the spermaceti; but the other oil is similar to what is called the train oil in the Whale.

I have endeavoured to discover the form of the crystals of different sorts of oil; but could never determine exactly what that was, because I could never find any of the crystals single, and by being always united, the natural form was not distinct.

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It is the adipose covering from all of the Whale kind that is brought home in square pieces, called flitches, and which, by being boiled, yields the oil on expression, leaving the cellular membrane. When these flitches have become in some degree putrid, there issues two sorts of oil; the first is pure, the last seems incorporated with part of the animal substance, which has become easy of solution from its putridity, forming a kind of butter. It is unctuous to the touch, ropy, coagulates or becomes harder by cold, swims upon water, not being soluble in it; and the pure oil, separating in the same manner from this, swims above all.

What remains, after all the oil is extracted, retains a good deal of its form, is almost wholly convertible into glue, and is sold for that purpose.

The cellular, or rather what should be called the uniting membrane in this order of animals, is similar to that in the quadruped; we find it uniting muscle to muscle, and muscle to bone, for their easy motion on one another.

The cellular membrane, which is the receptacle for the oil near the surface of the body is in general very different from that in the quadruped, as has been already observed.

Of the Skin.

The covering of this order of animals consists of a cuticle and cutis.

The cuticle is somewhat similar to that on the sole of the foot in the human species, and appears to be made up of a number of layers, which separate by slight putrefaction; but this I suspect arises in some degree from there being a succession of cuticles formed. It has no degree of elasticity or toughness,

but tears easily; nor do its fibres appear to have any particular direction. The internal stratum is tough and thick, and in the Spermaceti Whale its internal surface, when separated from the cutis, is just like coarse velvet, each pile standing firm in its place; but this is not so distinguishable in some of the others, although it appears rough from the innumerable perforations.

It is the cuticle that gives the colour to the animal; and in parts that are dark, I think, I have seen a dirty coloured substance washed away in the separation of the cuticle from the cutis, which must be a kind of rete mucosum.

The cutis in this tribe is extremely villous on its external surface, answering to the rough surface of the cuticle, and forming in some parts small ridges, similar to those on the human fingers and toes. These villi are soft and pliable; they float in water, and each is longer or shorter according to the size of the animal. In the Spermaceti Whale they were about a quarter of an inch long; in the Grampus, Bottle-nose and Piked Whales, much shorter; in all, they are extremely vascular.

The cutis seems to be the termination of the cellular membrane of the body more closely united, having smaller interstices, and becoming more compact. This alteration in the texture is so sudden as to make an evident distinction between what is solely connecting membrane, and skin, and is most evident in lean animals; for in the change from fat to lean, the skin does not undergo an alteration equal to what takes place in the adipose membrane, although it may be observed, that the skin itself is diminished in thickness. In fat animals the distinction between skin and cellular membrane is much less, the gradation from the one to the other seeming to be slower; for the cells of both membrane and skin being loaded with fat, the whole
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has more the appearance of one uniform substance. This uniformity of the adipose membrane and skin is most observable in the Whale, Seal, Hog, and the human species; and is not only visible in the raw but in the dressed hides; for in dressed skins the external is much more compact in texture than the inner surface, and is in common very tough.

In some animals the cutis is extremely thick, and in some parts much more so than others: where very thick, it appears to be intended as a defence against the violence of their own species or other animals. In most quadrupeds it is muscular, contracting by cold, and relaxing by heat. Many other stimulating substances make it contract; but cold is probably that stimulus by which it was intended to be generally affected.

The skin is extremely elastic in the greatest number of quadrupeds, and in its contracted state may be said to be rather too small for the body; by this elasticity it adapts itself to the changes which are constantly taking place in the parts, and it is from the want of it, that it becomes too large in some old animals. In all animals it is more elastic in some parts than others, especially in those where there is the greatest motion. How far these variations take place in the Whale I do not exactly know; but a loose elastic skin in this tribe would appear to be improper as an universal covering, considering the progressive motion of the animal, and the medium in which it moves; therefore it appears to be kept always on the stretch, by the adipose membrane being loaded with fat, which does not allow the skin to recede when cut. It is, however, more elastic at the setting on of the eyelids, round the opening of the prepuce, the nipples, the setting on of the fins, and under the jaw, to allow of motion in those parts; and

and here there is more reticular, and less adipose membrane. But in the Piked Whale there is probably one of the most striking instances of an elastic cuticular contraction: for though the whole skin of the fore part of the neck and breast of the animal, as far down as the middle of the belly, be extremely elastic; yet to render it still more so, it is ribbed longitudinally like a ribbed stocking, which gives an increased lateral elasticity. These ribs are, when contracted, about five-eighths of an inch broad, covered with the common skin of the animal; but in the hollow part of the rib, it is of a softer texture, with a thinner cuticle. This part is possessed of the greatest elasticity; why it should be so elastic is difficult to say, as it covers the thorax, which can never be increased in size; yet there must be some peculiar circumstance in the oeconomy of the species requiring this structure, which we as yet know nothing of.

The skin is intended for various purposes. It is the universal covering given for the defence of all kinds of animals; and that it might answer this purpose well, it is the seat of one of the senses.

Of the Mode of catching their Food.

The mouths of animals are the first parts to be considered respecting nourishment or food, and are so much connected with every thing relative to it, as not only to give good hints whether the food is vegetable or animal, but also respecting the particular kind of either, especially of animal food. The mouth not only receives the food, but is the immediate instrument for catching it. As it is a compound instrument in many animals, having parts of various constructions belonging to it, I shall at present consider it in this tribe no further than as connected

connected with their mode of catching the food, and adapting and disposing it for being swallowed. It is probable, that these animals do not require either a division of the food, or a mastication of it in the mouth, but swallow whatever they catch, whole; for we do not find any of them furnished with parts capable of producing either effect. The mouth in most of this tribe is well adapted for catching the food; the jaws spread as they go back, making the mouth proportionally wider than in many other animals.

There is a very great variety in the formation of the mouths of this tribe of animals, which we have many opportunities of knowing, from the head being often brought home when the other parts of the animal are rejected; a circumstance which frequently leaves us ignorant of the particular species to which it belonged.

Some catch their food by means of teeth, which are in both jaws, as the Porpoise and Grampus; in others, they are only in one jaw, as in the Spermaceti Whale; and in the large Bottle-nose Whale, described by DALE, there are only two small teeth in the anterior part of the lower jaw. In the Narwhale only two tusks in the fore part of the upper jaw*; while in some others there are none at all. In those which have teeth in both jaws, the number in each varies considerably; the small Bottle-nose had forty-six in the upper, and fifty in the lower; and in the jaws of others there are only five or six in each.

The teeth are not divisible into different classes, as in quadrupeds; but are all pointed teeth, and are commonly a good deal similar. Each tooth is a double cone, one point being

* I call these tusks to distinguish them from common teeth. A tusk is that kind of tooth which has no bounds set to its growth, excepting by abrasion, as the tusk of the Elephant, Boar, Sea-horse, Manatee, &c.

fastened

fastened in the gum, the other projecting: they are, however, not all exactly of this shape. In some species of Porpoise the fang is flattened, and thin at its extremity; in the Spermaceti Whale the body of the tooth is a little curved towards the back part of the mouth; this is also the case in some others. The teeth are composed of animal substance and earth, similar to the bony part of the teeth in quadrupeds. The upper teeth are commonly worn down upon the inside, the lower on the outside; this arises from the upper jaw being in general the largest.

The situation of the teeth, when first formed, and their progress afterwards, as far as I have been able to observe, is very different in common from those of the quadruped. In the quadruped the teeth are formed in the jaw, almost surrounded by the alveoli, or sockets, and rise in the jaw as they increase in length; the covering of the alveoli being absorbed, the alveoli afterwards rise with the teeth, covering the whole fang; but in this tribe the teeth appear to form in the gum, upon the edge of the jaw, and they either sink in the jaw as they lengthen, or the alveoli rise to inclose them: this last is most probable, since the depth of the jaw is also increased, so that the teeth appear to sink deeper and deeper in the jaw. This formation is readily discovered in jaws not full grown; for the teeth increase in number as the jaw lengthens, as in other animals. The posterior part of the jaw becoming longer, the number of teeth in that part increases, the sockets becoming shallower and shallower, and at last being only a slight depression.

It would appear, that they do not shed their teeth, nor have they new ones formed similar to the old, as is the case with most other quadrupeds, and also with the Alligator. I have never been able to detect young teeth under the roots of the old ones; and indeed the situation in which they are first formed

makes it in some degree impossible, if the young teeth follow the same rule in growing with the original ones, as they probably do in most animals.

If it is true, that the Whale tribe do not shed their teeth, in what way are they supplied with new ones, corresponding in size with the increased size of the jaw? It would appear, that the jaw, as it increases posteriorly, decays at the symphysis, and while the growth is going on, there is a constant succession of new teeth, by which means the new-formed teeth are proportioned to the jaw. The same mode of growth is evident in the Elephant, and in some degree in many fish; but in these last the absorption of the jaw is from the whole of the outside along where the teeth are placed. The depth of the alveoli seems to prove this, being shallow at the back part of the jaw, and becoming deeper towards the middle, where they are the deepest, the teeth there having come to the full size. From this forwards they are again becoming shallower, the teeth being smaller, the sockets wasting, and at the symphysis there are hardly any sockets at all. This will make the exact number of teeth in any species uncertain.

Some genera of this tribe have another mode of catching their food, and retaining it till swallowed, which is by means of the substance called Whalebone. Of this there are two kinds known; one very large, probably from the largest Whale yet discovered; the other from a smaller species.

This whalebone, which is placed on the inside of the mouth, and attached to the upper jaw, is one of the most singular circumstances belonging to this species, as they have most other parts in common with quadrupeds. It is a substance, I believe, peculiar to the Whale, and of the same nature as horn, which I shall use as a term to express what constitutes

tutes hair, nails, claws, feathers, &c. it is wholly composed of animal substance, and extremely elastic *.

Whalebone consists of thin plates of some breadth, and in some of very considerable length, their breadth and length in some degree corresponding to one another; and when longest they are commonly the broadest, but not always so. (See Tab. XXII.) These plates are very different in size in different parts of the same mouth, more especially in the large Whalebone Whale, whose upper jaw does not pass parallel upon the under, but makes an arch, the semidiameter of which is about one-fourth of the length of the jaw. The head in my possession is nineteen feet long, the semidiameter not quite five feet: if this proportion is preserved, those Whales which have whalebone fifteen feet long must be of an immense size.

These plates are placed in several rows, encompassing the outer skirts of the upper jaw, similar to teeth in other animals. They stand parallel to each other, having one edge towards the circumference of the mouth, the other towards the center or cavity. They are placed near together in the Piked Whale, not being a quarter of an inch asunder where at the greatest distance, yet differing in this respect in different parts of the same mouth; but in the great Whale the distances are more considerable.

The outer row is composed of the longest plates; and these are in proportion to the different distances between the two jaws, some being fourteen or fifteen feet long, and twelve or fifteen inches broad; but towards the anterior and posterior part of the mouth, they are very short: they rise for half a foot or more, nearly of equal breadths, and afterwards shelve off from their inner side until they come near to a point at the

* From this it must appear, that the term bone is an improper one.

outer: the exterior of the inner rows are the longest, corresponding to the termination of the declivity of the outer, and become shorter and shorter till they hardly rise above the gum.

The inner rows are closer than the outer, and rise almost perpendicularly from the gum, being longitudinally straight, and have less of the declivity than the outer. The plates of the outer row laterally are not quite flat, but make a serpentine line, more especially in the Piked Whale the outer edge is thicker than the inner. All round the line made by their outer edges, runs a small white bead, which is formed along with the whalebone, and wears down with it. The smaller plates are nearly of an equal thickness upon both edges. In all of them, the termination is in a kind of hair, as if the plate was split into innumerable small parts, the exterior being the longest and strongest.

The two sides of the mouth composed of these rows meet nearly in a point at the tip of the jaw, and spread or recede laterally from each other as they pass back; and at their posterior ends, in the Piked Whale, they make a sweep inwards, and come very near each other, just before the opening of the œsophagus. In the Piked Whale there were above three hundred in the outer rows on each side of the mouth. Each layer terminates in an oblique surface, which obliquity inclines to the roof of the mouth, answering to the gradual diminution of their length; so that the whole surface, composed of these terminations, forms one plane rising gradually from the roof of the mouth; from this obliquity of the edge of the outer row, we may in some measure judge of the extent of the whole base, but not exactly, as it makes a hollow curve, which increases the base.

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The whole surface resembles the skin of an animal covered with strong hair, under which surface the tongue must immediately lie, when the mouth is shut; it is of a light-brown colour in the Piked Whale, and is darker in the large Whale.

In the Piked Whale, when the mouth is shut, the projecting whalebone remains entirely on the inside of the lower jaw, the two jaws meeting every where along their surface; but how this is effected in the large Whale I do not certainly know, the horizontal plane made by the lower jaw being straight, as in the Piked Whale; but the upper jaw being an arch cannot be hid by the lower. I suppose, therefore, that a broad upper lip, meeting as low as the lower jaw, covers the whole of the outer edges of the exterior rows.

The whalebone is continually wearing down, and renewing in the same proportion, except when the animal is growing it is renewed faster, and in proportion to the growth.

The formation of the whalebone is extremely curious, being in one respect similar to that of the hair, horns, spurs, &c.; but it has besides another mode of growth and decay, equally singular.

These plates form upon a thin vascular substance, not immediately adhering to the jaw-bone; but having a more dense substance between, which is also vascular. This substance, which may be called the nidus of the whalebone, sends out (the above) thin broad processes, answering to each plate, on which the plate is formed, as the Cock's spur or the Bull's horn, on the bony core, or a tooth on its pulp; so that each plate is necessarily hollow at its growing end, the first part of the growth taking place on the inside of this hollow.

Besides

Besides this mode of growth, which is common to all such substances, it receives additional layers on the outside, which are formed upon the above-mentioned vascular substance extended along the surface of the jaw. This part also forms upon it a semi-horny substance between each plate, which is very white, rises with the whalebone, and becomes even with the outer edge of the jaw, and the termination of its outer part forms the bead above mentioned. This intermediate substance fills up the spaces between the plates as high as the jaw, acts as abutments to the whalebone, or is similar to the alveolar processes of the teeth, keeping them firm in their places. (See Tab. XXIII.)

As both the whalebone and intermediate substance are constantly growing, and as we must suppose a determined length necessary, a regular mode of decay must be established, not depending entirely on chance, or the use it is put to.

In its growth, three parts appear to be formed; one from the rising core, which is the center, a second on the outside, and a third being the intermediate substance. These appear to have three stages of duration; for that which forms on the core, I believe, makes the hair, and that on the outside makes principally the plate of whalebone; this, when got a certain length, breaks off, leaving the hair projecting, becoming at the termination very brittle; and the third, or intermediate substance, by the time it rises as high as the edge of the skin of the jaw, decays and softens away like the old cuticle of the sole of the foot when steeped in water.

The use of the whalebone, I should believe, is principally for the retention of the food till swallowed; and do suppose the fish they catch are small, when compared with the size of the mouth.

The œsophagus, as in other animals, begins at the fauces, or posterior part of the mouth; and, although circular at this part, is soon divided into two passages by the epiglottis passing across it, as will be described hereafter. Below its attachment to the trachea, it passes down in the posterior mediastinum, at some distance from the spine, to which it is attached by a broad part of the same membrane, and its anterior surface makes the posterior part of a cavity behind the pericardium.

Passing through the diaphragm it enters the stomach, and is lined with a very thick, soft, and white cuticle, which is continued into the first cavity of the stomach.

The inner, or true coat, is white, of a considerable density, and not muscular; but thrown into large longitudinal folds by the contraction of the muscular fibres of the œsophagus, which are very strong. It is very glandular; for on its inner surface, especially near the fauces, orifices of a vast number of glands are visible.

The œsophagus is larger in proportion to the bulk of the animal than in the quadruped, although not so much so as it usually is in fish, which we may suppose swallow their food much in the same way. In the Piked Whale it was three inches and an half wide.

The stomach, as in other animals, lies on the left side of the body, and terminates in the pylorus towards the right.

The duodenum passes down on the right side, very much as in the human subject, excepting that it is more exposed from the colon not crossing it. It lies on the right kidney, and then passes to the left side behind the ascending part of the colon and root of the mesentery, comes out on the left side, and getting on the edge of the mesentery becomes a loose intestine,

testine, forming the jejunum. In this course behind the mesentery it is exposed, as in most quadrupeds, not being covered by it, as in the human. The jejunum and ilium pass along the edge of the mesentery downwards to the lower part of the abdomen. The ilium near the lower end makes a turn towards the right side, and then mounting upwards, round the edge of the mesentery, passes a little way on the right, as high as the kidney, and there enters the colon, or cæcum. The cæcum lies on the lower end of the kidney, considerably higher than in the human body, which renders the ascending part of the colon short. The cæcum is about seven inches long, and more like that of the Lion or Seal than of any other animal I know.

The colon passes obliquely up the right side, a little towards the middle of the abdomen; and when as high as the stomach, crosses to the left, and acquires a broad mesocolon: at this part it lies upon the left kidney, and in its passage down gets more and more to the middle line of the body. When it has reached the lower part of the abdomen, it passes behind the uterus, and along with the vagina, in the female; between the two testicles, and behind the bladder and root of the penis, in the male, bending down to open on what is called the belly of the animal; and in its whole course it is gently convoluted. In those which have no cæcum, and therefore can hardly be said to have a colon, the intestine before its termination in the rectum makes the same kind of sweep round the other intestines, as the colon does where there is a cæcum.

The intestines are not large for the size of the animal, not being larger in those of eighteen or twenty-four feet long than in the Horse, the colon not much more capacious than the jejunum and ilium, and very short; a circumstance common to carni-

vorous animals. In the Piked Whale, the length from the stomach to the cæcum is 28 yards and an half, length of cæcum seven inches, of the colon to the anus two yards and three quarters. The small intestines are just five times the length of the animal, the colon with the cæcum a little more than one-half the length.

Those parts that respect the nourishment of this tribe do not all so exactly correspond as in land animals; for in these one in some degree leads to the other. Thus the teeth in the ruminating tribe point out the kind of stomach, cæcum, and colon; while in others, as the Horse, Hare, Lion, &c. the appearances of the teeth only give us the kind of colon and cæcum; but in this tribe, whether teeth or no teeth, the stomachs do not vary much, nor does the circumstance of cæcum seem to depend on either teeth or stomach. The circumstances by which, from the form of one part we judge what others are, fail us here; but this may arise from not knowing all the circumstances. The stomach, in all that I have examined, consists of several bags, continued from the first on the left towards the right, where the last terminates in duodenum. The number is not the same in all; for in the Porpoise, Grampus, and Piked Whale, there are five; in the Bottle-nose seven. Their size respecting one another differs very considerably; so that the largest in one species may in another be only the second. The two first in the Porpoise, Bottle-nose, and Piked Whale, are by much the largest; the others are smaller, although irregularly so.

The first stomach has, I believe, in all very much the shape of an egg, with the small end downwards. It is lined everywhere with a continuation of the cuticle from the œsophagus. In the Porpoise the œsophagus enters the superior end of the sto-

mach. In the Piked Whale its entrance is a little way on the posterior part of the upper end, and is oblique.

The second stomach in the Piked Whale is very large, and rather longer than the first. It is of the shape of the Italic S, passing out from the upper end of the first on its right side, by nearly as large a beginning as the body of the bag. In the Porpoise it by no means bears the same proportion to the first, and opens by a narrower orifice; then passing down along the right side of the first stomach, it bends a little outwards at the lower end, and terminates in the third. Where this second stomach begins, the cuticle of the first ends. The whole of the inside of this stomach is thrown into unequal rugæ, appearing like a large irregular honeycomb. In the Piked Whale the rugæ are longitudinal, and in many places very deep, some of them being united by cross bands; and in the Porpoise the folds are very thick, massy, and indented into one another. This stomach opens into the third by a round contracted orifice, which does not seem to be valvular.

The third stomach is by much the smallest, and appears to be only a passage between the second and fourth. It has no peculiar structure on the inside, but terminates in the fourth by nearly as large an opening as its beginning. In the Porpoise it is not above one, and in the Bottle-nose about five inches long.

The fourth stomach is of a considerable size; but a good deal less than either the first or second. In the Piked Whale it is not round, but seems flattened between the second and fifth. In the Porpoise it is long, passing in a serpentine course almost like an intestine. The internal surface is regular, but villous, and opens on its right side into the fifth, by a round opening smaller than the entrance from the third.

The fifth stomach is in the Piked Whale round, and in the Porpoise oval; it is small, and terminates in the pylorus, which has little of a valvular appearance. Its coats are thinner than those of the fourth, having an even inner surface, which is commonly tinged with bile.

The Piked Whale and, I believe, the large Whalebone Whale, have a cæcum; but it is wanting in the Porpoise, Grampus, and Bottle-nose Whale.

The structure of the inner surface of the intestine is in some very singular, and different from that of the others.

The inner surface of the duodenum in the Piked Whale is thrown into longitudinal rugæ, or valves, which are at some distance from each other, and these receive lateral folds. The duodenum in the Bottle-nose swells out into a large cavity, and might almost be reckoned an eighth stomach; but as the gall ducts enter it I shall call it duodenum.

The inner coat of the jejunum, and ilium, appears in irregular folds, which may vary according as the muscular coat of the intestine acts: yet I do not believe, that their form depends intirely on that circumstance, as they run longitudinally, and take a serpentine course when the gut is shortened by the contraction of the longitudinal muscular fibres. The intestinal canal of the Porpoise has several longitudinal folds of the inner coat passing along it, through the whole of its length. In the Bottle-nose the inner coat, through nearly the whole track of the intestine, is thrown into large cells, and these again subdivided into smaller; the axis of which cells is not perpendicular to a transverse section of the intestine, but oblique, forming pouches with the mouths downwards, and acting almost like valves, when any thing is attempted to be passed in a contrary direction: they begin faintly in the duodenum, before it makes

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its quick turn, and terminate near the anus. The colon and rectum have the rugæ very flat, which seems to depend entirely on the contraction of the gut.

The rectum near the anus appears, for four or five inches, much contracted, is glandular, covered by a soft cuticle, and the anus small.

I never found any air in the intestines of this tribe; nor indeed in any of the aquatic animals.

The mesenteric artery anastomoses by large branches.

There is a considerable degree of uniformity in the liver of this tribe of animals. In shape it nearly resembles the human, but is not so thick at the base, nor so sharp at the lower edge, and is probably not so firm in its texture. The right lobe is the largest and thickest, its falciform ligament broad, and there is a large fissure between the two lobes, in which the round ligament passes. The liver towards the left is very much attached to the stomach, the little epiploon being a thick substance. There is no gall-bladder; the hepatic duct is large, and enters the duodenum about seven inches beyond the pylorus.

The pancreas is a very long, flat body, having its left end attached to the right side of the first cavity of the stomach: it passes across the spine at the root of the mesentery, and near to the pylorus joins the hollow curve of the duodenum, along which it is continued, and adheres to that intestine, its duct entering that of the liver near the termination in the gut.

Although this tribe cannot be said to ruminate, yet in the number of stomachs they come nearest to that order; but here I suspect that the order of digestion is in some degree inverted. In both the ruminants, and this tribe, I think it must be allowed that the first stomach is a reservoir. In the ruminants the precise use of the second and third stomachs is perhaps not known; but

but digestion is certainly carried on in the fourth; while in this tribe, I imagine, digestion is performed in the second, and the use of the third and fourth is not exactly ascertained.

The cæcum and colon do not assist in pointing out the nature of the food and mode of digestion in this tribe. The Porpoise which has teeth, and four cavities to the stomach, has no cæcum, similar to some land animals, as the Bear, Badger, Raccoon, Ferret, Polecat, &c.; neither has the Bottle-nose a cæcum which has only two small teeth in the lower jaw; and the Piked Whale, which has no teeth, has a cæcum, almost exactly like the Lion, which has teeth and a very different kind of stomach.

The food of the whole of this tribe, I believe, is fish; probably each may have a particular kind, of which it is fondest, yet does not refuse a variety. In the stomach of the large Bottle-nose, I found the beaks of some hundreds of Cuttle-fish. In the Grampus I found the tail of a Porpoise; so that they eat their own genus. In the stomach of the Piked Whale, I found the bones of different fish, but particularly those of the Dog-fish. From the size of the œsophagus we may conclude, that they do not swallow fish so large in proportion to their size as many fish do, that we have reason to believe take their food in the same way: for fish often attempt to swallow what is larger than their stomachs can at one time contain, and part remains in the œsophagus till the rest is digested.

The epiploon on the whole is a thin membrane; on the right side it is rather a thin net-work, though on the left it is a complete membrane, and near to the stomach of the same side becomes of a considerable thickness, especially between the two first bags of the stomach. It has little or no fat, except
what

what slightly covers the vessels in particular parts. It is attached forwards, all along to the lower part of the different bags constituting the stomach, and on the right to the root of the mesentery, between the stomach and transverse arch of the colon, first behind to the transverse arch of the colon and root of the mesentery, then to the posterior surface of the left or first bag of the stomach, behind the anterior attachment. In some of this tribe there is the usual passage behind the vessels going to the liver, common to all quadrupeds I am acquainted with; but in others, as the small Bottle-nose, there is no such passage, which by the cavity behind the stomach in the epiploon of this animal becomes a circumscribed cavity.

The spleen is involved in the epiploon, and is very small for the size of the animal. There are in some, as the Porpoise, one or two small ones, about the size of a nutmeg, often smaller, placed in the epiploon behind the other. These are sometimes met with likewise in the human body.

The kidneys in the whole of this tribe of animals are conglomerated, being made up of smaller parts, which are only connected by cellular membrane, blood-vessels, and ducts, or infundibula; but not partially connected by continuity of substance, as in the human body, the Ox, &c.: every portion is of a conical figure, whose apex is placed towards the center of the kidney, the base making the external surface; and each is composed of a cortical and tubular substance, the tubular terminating in the apex, which apex makes the mamilla. Each mamilla has an infundibulum, which is long, and at its beginning wide, embracing the base of the mamilla, and becoming smaller. These infundibula unite at last, and form the ureter. The whole kidney is an oblong flat body, broader and thicker at the upper end than the lower, and has the appearance

appearance of being made up of different parts placed close together, almost like the pavement of a street.

The ureter comes out at the lower end, and passes along to the bladder, which it enters very near the urethra.

The bladder is oblong, and small for the size of the animal. In the female the urethra passes along to the external sulcus or vulva, and opens just under the clitoris, much as in the human subject.

Whether being inhabitants of the water makes such a construction of kidney necessary I cannot say; yet one must suppose it to have some connection with such situation, since we find it almost uniformly take place in animals inhabiting the water, whether wholly, as this tribe, or occasionally, as the Manatee, Seal, and White Bear: there is, however, the same structure in the Black Bear, which, I believe, never inhabits the water. This, perhaps, should be considered in another light, as nature keeping up to a certain uniformity in the structure of similar animals; for the Black Bear in construction of parts is, in every other respect as well as this, like the White Bear.

The capsulæ renales are small for the size of the animal, when compared to the human, as indeed they are in most animals. They are flat, and of an oval figure; the right lies on the lower and posterior part of the diaphragm somewhat higher than the kidney; the left is situated lower down, by the side of the aorta, between it and the left kidney. They are composed of two substances; the external having the direction of its fibres or parts towards the center; the internal seeming more uniform, and not having so much of the fibrous appearance.

The blood of animals of this order is, I believe, similar to that of quadrupeds; but I have an idea, that the red globules are

are in larger proportion. I will not pretend to determine how far this may assist in keeping up the animal heat; but as these animals may be said to live in a very cold climate or atmosphere, and such as readily carries off heat from the body, they may want some help of this kind.

It is certain that the quantity of blood in this tribe and in the Seal is comparatively larger than in the quadruped, and therefore probably amounts to more than that of any other known animal.

This tribe differs from fish in having the red blood carried to the extreme parts of the body, similar to the quadruped.

The cavity of the thorax is composed of nearly the same parts as in the quadruped; but there appears to be some difference, and the varieties in the different genera are greater.

The general cavity is divided into two, as in the quadruped, by the heart and mediastinum.

The heart in this tribe, and in the Seal, is probably larger in proportion to their size than in the quadruped, as also the blood-vessels, more especially the veins.

The heart is inclosed in its pericardium, which is attached by a broad surface to the diaphragm, as in the human body. It is composed of four cavities*, two auricles, and two ventricles: it is more flat than in the quadruped, and adapted to the shape of the chest. The auricles have more fasciculæ, and these pass more across the cavity from side to side than in many other animals; besides, being very muscular, they are very elastic,

* As the circulation is a permanent part of the constitution respecting the class to which the animal belongs, and as the kind of heart corresponds with the circulation, these should be considered in the classing of animals. Thus we have animals whose hearts have only one cavity, others with two, three, and four cavities.

for being stretched they contract again very considerably. There is nothing uncommon or particular in the structure of the ventricles, in the valves of the ventricles, or in that of the arteries.

The general structure of the arteries resembles that of other animals; and where parts are nearly similar, the distribution is likewise similar. The aorta forms its usual curve, and sends off the carotid and subclavian arteries.

Animals of this tribe, as has been observed, have a greater proportion of blood than any other known, and there are many arteries apparently intended as reservoirs, where a larger quantity of arterial blood seemed to be required in a part, and vascularity could not be the only object. Thus we find, that the intercostal arteries divide into a vast number of branches, which run in a serpentine course between the pleura, ribs, and their muscles, making a thick substance somewhat similar to that formed by the spermatick artery in the Bull. Those vessels, every where lining the sides of the thorax, pass in between the ribs near their articulation, and also behind the ligamentous attachment of the ribs, and anastomose with each other. The medulla spinalis is surrounded with a net-work of arteries in the same manner, more especially where it comes out from the brain, where a thick substance is formed by their ramifications and convolutions; and these vessels most probably anastomose with those of the thorax.

The subclavian artery in the Piked Whale, before it passes over the first rib, sends down into the chest arteries which assist in forming the plexus on the inside of the ribs; I am not certain but the internal mammary arteries contribute to form the anterior part of this plexus. The motion of the blood in such must be very slow; the use of which we do not readily see. The descending aorta sends off the intercostals, which are very large, and give branches to this plexus; and when it has reached the abdomen, it sends off, as in the quadruped, the different

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branches to the viscera, and the lumbar arteries, which are likewise very large for the supply of that vast mass of muscles which moves the tail.

In our examination of particular parts, the size of which is generally regulated by that of the whole animal, if we have only been accustomed to see them in those which are small or middle-sized, we behold them with astonishment in animals so far exceeding the common bulk as the Whale. Thus the heart and aorta of the Spermaceti Whale appeared prodigious, being too large to be contained in a wide tub, the aorta measuring a foot in diameter. When we consider these as applied to the circulation, and figure to ourselves, that probably ten or fifteen gallons of blood are thrown out at one stroke, and moved with an immense velocity through a tube of a foot diameter, the whole idea fills the mind with wonder.

The veins, I believe, have nothing particular in their structure, excepting in parts requiring a peculiarity, as in the folds of the skin on the breast in the Piked Whale, where their elasticity was to be increased.

Of the Larynx.

The larynx in most animals living on land is a compound organ, adapted both for respiration, deglutition, and sound, which last is produced in the actions of respiration; but in this tribe the larynx, I suppose, is only adapted to respiration, as we do know that they have any mode of producing sound.

It is composed of os hyoides, thyroid, cricoid, and two arytenoid cartilages, with the epiglottis. It varies very much in structure and size, when compared in animals of different genera. These cartilages were much smaller in the Bottle-nose of twenty-four feet long, than in the Piked Whale of seventeen feet, while the os hyoides was much larger.

In

In the Bottle-nose, the os hyoides is composed of three bones, besides two whose ends are attached to it, being placed above the os hyoides, making five in all. In the Porpoise, Piked Whale, &c. it is but one bone, slightly bent, having a broad thin process passing up, which is a little forked: it has no attachment to the head by means of other bones, as in many quadrupeds.

The thyroid cartilage in the Piked Whale is broad from side to side, but not from the upper to the lower part: it has two lateral processes, which are long, and pass down the outside of the cricoid, near to its lower end, and are joined to it much as in the human subject. These differ in shape in different animals of this tribe.

The cricoid cartilage is broad and flat, making the posterior and lateral part of the larynx, and is much deeper behind, and laterally, than before. It is extremely thick and strong, flattened on the posterior surface, and hollowed from the upper edge to the lower. It terminates by a thick edge on the posterior part above, but irregularly at the lower edge, in the cartilages of the larynx.

The two arytenoid cartilages are extremely projecting, and united to each other till near their ends; are articulated on the upper edge of the cricoid, but send down a process, which passes on the inside of the cricoid, being attached to a bag in the Piked Whale, which is formed below the thyroid and before the cricoid cartilages; they cross the cavity of the larynx obliquely, making the passage, at the upper part, a groove between them: the cavity at this place swells out laterally, but is very narrow between the anterior and posterior surfaces. The passage above between the arytenoid and thyroid cartilages is wide from side to side, and is continued down on the outside of the processes of

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the arytenoid cartilage, as well as between them, ending below the thyroid, which is folliculated on its inner surface on the fore part of the cricoid cartilage.

The epiglottis makes a third part of the passage, and compleats the glottis by forming it into a canal, in several of this tribe; but in the Piked Whale it was not attached to the two arytenoid cartilages, but only in contact, or inclosing them at their base, so as to make them form a complete canal.

I could not observe any thing like a thyroid gland.

From the glottis and epiglottis being so connected as to make but one canal, and from the thyroid and cricoid cartilages being so flattened in some between the anterior and posterior surface, the passage through these parts is very small or contracted; but the trachea swells out again into a very considerable size. Its larger branches are in proportion to the trunk, and enter the lungs at the upper end along with the blood-vessels.

Of the Lungs.

The lungs are two oblong bodies, one on each side of the chest, and are not divided into smaller lobes, as in the human subject. They are of considerable length, but not so deep between the fore and back part, as in the quadruped, from the heart being broad, flat, and of itself filling up the fore part of the chest. They pass further down on the back part than in the quadruped, by which their size is increased, and rise higher up in the chest than the entrance of the vessels, coming to a point at the upper end. From the entrance of the vessels they are connected downwards, along their whole inner edge, by a strong attachment (in which there are in some lymphatic glands) to

the posterior mediastinum. The lungs are extremely elastic in their substance, even so much so as to squeeze out any air that may be thrown into them, and to become almost at once a solid mass, having a good deal the appearance, consistence, and feel of an ox's spleen. The branches of the bronchiæ which ramify into the lungs have not the cartilages flat, but rather rounded; a construction which admits of greater motion between each.

The pulmonary cells are smaller than in quadrupeds, which may make less air necessary, and they communicate with each other; which those of the quadruped do not; for by blowing into one branch of the trachea, not only the part to which it immediately goes, but the whole lungs are filled.

As the ribs in this tribe do not completely make the cavity of the thorax, the diaphragm has not the same attachments as in the quadruped, but is connected forwards to the abdominal muscles, which are very strong, being a mixture of muscular and tendinous fibres.

The position of the diaphragm is less transverse than in the quadruped, passing more obliquely backwards, and coming very low on the spine, and higher up before; which makes the chest longest in the direction of the animal at the back, and gives room for the lungs to be continued along the spine.

The parts immediately concerned in inspiration are extremely strong; the diaphragm remarkably so. The reason of this must at once appear; it necessarily requiring great force to expand in a dense medium like water, especially too when the vacuity is to be filled with one which is rarer, and is to water a species of vacuum, the pressure being much greater on the external surface than the counter-pressure from within. But expiration on the other hand must be much more easily performed;

formed; the natural elasticity of the parts themselves, with the pressure of the water on the external surface of the body, being greater than the resistance of the air from within, will both tend to produce expiration without any immediate action of muscles.

The diaphragm, in these animals, appears to be the principal agent in inspiration; and the cavity of the thorax not being entirely surrounded by bony parts, is of course less easily expanded, and the apparatus for its expansion in all directions, as in the quadruped, does not exist here.

The Blow-hole, or Passage for the Air.

As the nose in every animal that breathes air is a common passage for the air, and is also the organ of smelling; I shall describe it in this tribe as instrumental to both these purposes.

There is a variety in some species of this animal which is, I believe, peculiar to this order; that is, the want of the sense of smelling; none of those which I have yet examined having that sense, except the two kinds of Whalebone Whale: such of course have neither the olfactory nerves, nor the organ; therefore, in them, the nostrils are intended merely for respiration; but others have the organ placed in this passage as in other animals.

The membranous portion of the posterior nostrils is one canal; but when in the bony part, in most of them, it is divided into two; the Spermaceti Whale, however, is an exception. In those which have it divided, it is in some continued double through the anterior soft parts, opening by two orifices, as in the Piked Whale; but in others, it unites again in the membranous part, making externally only one orifice, as in the
Porpoise,

Porpoise, Grampus, and Bottle-nose. At its beginning in the fauces, it is a roundish hole, surrounded by a strong sphincter muscle, for grasping the epiglottis; beyond this, the canal becomes larger, and opens into the two passages in the bones of the head. This part is very glandular, being full of follicles, whose ducts ramify in the surrounding substance, which appears fatty and muscular like the root of the tongue, and these ramifications communicate with one another, and contain a viscid slime.

In the *Spermaceti Whale*, which has a single canal, it is thrown a little to the left side. After these canals emerge from the bones near the external opening, they become irregular, and have several fulci passing out laterally, of irregular forms, with corresponding eminences. The structure of these eminences is muscular and fatty, but less muscular than the tongue of a quadruped.

In the *Porpoise* there are two fulci on each side; two large and two small, with corresponding eminences of different shapes, the large ones being thrown into folds. The *Spermaceti Whale* has the least of this structure; the external opening in it comes farther forwards towards the anterior part of the head, and is consequently longer than in others of this order. Near to its opening externally, it forms a large fulcus, and on each side of this canal is a cartilage, which runs nearly its whole length. In all that I have examined, this canal, forwards from the bones, is intirely lined with a thick cuticle of a dark colour.

In those which have only one external opening, it is transverse, as in the *Porpoise*, *Grampus*, *Bottle-nose* and *Spermaceti Whale*, &c.; where double, they are longitudinal, as in the *Piked Whale*, and the large *Whalebone Whale*. These
openings

openings form a passage for the air in respiration to and from the lungs; for it would be impossible for these animals to breathe air through the mouth; indeed, I believe, the human species alone breathe by the mouth, and in them it is mostly from habit; for in quadrupeds the epiglottis conducts the air into the nose.

In the whole of this tribe, the situation of the opening on the upper surface of the head is well adapted for this purpose, being the first part that comes to the surface of the water in the natural progressive motion of the animal; therefore it is to be considered principally as a respiratory organ, and where it contains the organ of smell, that is only secondary.

As the animals of this order do not live in the medium which they inspire, the organs conducting the air to the lungs are in some sort particularly constructed, that the water in which they live may not interfere with the air they breathe.

The projecting glottis, which has been described, passes into the posterior nostrils, by which means it crosses the fauces, dividing them into two passages. The enlargement at the termination of the glottis, observed in some of them, would seem to be intended to prevent its retraction; but, as it seems confined to the Porpoise and Grampus, it may, perhaps, in them answer some other purpose.

The beginning of the posterior nostrils, which answers to the palatum molle in the quadruped, having a sphincter, the glottis is grasped by it, which renders its situation still more secure, and the passages through the head, across the fauces and along the trachea, are rendered one continued canal; this union of glottis and epiglottis with the posterior nostril, making only a kind of joint, admits of motion, and of dilatation and contraction of the fauces, in deglutition, from the epiglottis moving more in or out of the posterior nostril.

This

This construction of parts answers a purpose similar to that of the epiglottis in the quadruped; it may be considered as the epiglottis and the arytenoid cartilages joining, to make a tubular or cylindrical epiglottis, instead of a valvular one.

The reasons why there should be so peculiar a construction of parts do not at first appear; but we certainly see by it an absolute guard placed upon the lungs, that no water should get into them.

This tribe being without the projecting tongue of the quadruped, and wanting its extensive motion, and the power of sucking things into the mouth, may probably require the construction between the air and lungs to be more perfect; but how far it is so, I will not pretend to say.

The size of the Brain differs much in different genera of this tribe, and likewise in the proportion it bears to the bulk of the animal. In the Porpoise, I believe, it is largest, and perhaps in that respect comes nearest to the human.

The size of the cerebellum in proportion to that of the cerebrum is smaller in the human subject than in any animal with which I am acquainted. In many quadrupeds, as the Horse, Cow, &c. the disproportion in size between cerebellum and cerebrum is not great, and in this tribe it is still less; yet not so small as in the bird, &c.

The whole brain in this tribe is compact, the anterior part of the cerebrum not projecting so far forwards as in either the quadruped or in the human subject; neither is the medulla oblongata so prominent but flat, lying in a kind of hollow made by the two lobes of the cerebellum.

The brain is composed of cortical and medullary substances, very distinctly marked; the cortical being, in colour, like the tubular substance of a kidney; the medullary, very white.

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These substances are nearly in the same proportion as in the human brain. The two lateral ventricles are large, and in those that have olfactory nerves are not continued into them as in many quadrupeds; nor do they wind so much outwards as in the human subject, but pass close round the posterior ends of the thalami nervorum opticom. The thalami themselves are large; the corpora striata small; the crura of the fornix are continued along the windings of the ventricles, much as in the human subject. The plexus choroides is attached to a strong membrane, which covers the thalami nervorum opticom, and passes through the whole course of the ventricle, much as in the human subject.

The substance of the brain is more visibly fibrous than I ever saw it in any other animal, the fibres passing from the ventricles as from a center to the circumference, which fibrous texture is also continued through the cortical substance. The whole brain in the Piked Whale weighed four pounds ten ounces.

The nerves going out from the brain, I believe, are similar to those of the quadruped, except in the want of the olfactory nerves in the genus of the Porpoise.

The medulla spinalis is much smaller in proportion to the size of the body than in the human species, but still bears some proportion to the quantity of brain; for in the Porpoise, where the brain is largest, the medulla spinalis is largest; yet this did not hold good in the Spermaceti Whale, the size of the medulla spinalis appearing to be proportionally larger than the brain, which was small when compared to the size of the animal. It has a cortical part in the center, and terminates about the twenty-fifth vertebra, beyond which is the cauda equina, the dura mater going no lower. The nerves which go
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off from the medulla spinalis are more uniform in size than in the quadruped, there being no such inequality of parts, nor any extremities to be supplied, except the fins.

The medulla spinalis is more fibrous in its structure than in other animals; and when an attempt is made to break it longitudinally, it tears with a fibrous appearance, but transversely it breaks irregularly.

The dura mater lines the skull, and forms in some the three processes answerable to the divisions of the brain, as in the human subject; but in others, this is bone. Where it covers the medulla spinalis, it differs from all the quadrupeds I am acquainted with, inclosing the medulla closely, and the nerves immediately passing out through it at the lower part, as they do at the upper, so that the cauda equina, as it forms, is on the outside of the dura mater.

As the Organs of Sense are variously formed in different animals, fitted for the various modes of impression; and as the modes are either increased or varied, according to circumstances which make no part of the sense itself, but which are necessary for the oeconomy of the animal, we find the senses in this tribe varied in their construction, and in some a sense is even wholly wanting.

The organs of sense, which appear to be adapted to every mode of life, are those of touch and taste; but those of smell, sight, and hearing, probably require to be varied according to circumstances. Thus smell may be increased by a mode of impregnation, hearing by the vibration of different mediums, and sight by the different powers of refraction of different mediums; therefore, as animals are intended by nature to be differently circumstanced, so are the senses formed.

Of the Sense of Touch.

The cutis in this tribe appears, in general, particularly well calculated for sensation; the whole surface being covered with villi, which are so many vessels, and we must suppose, nerves. Whether this structure is only necessary for acute sensation, or whether it is necessary for common sensation, where the cuticle is thick, and consisting of many layers, I do not know. We may observe, that where it is necessary the sense of touch should be accurate, the villi are usually thick and long, which probably is necessary, because in most parts of the body, where the more acute sensations of touch are required, such parts are covered by a thick cuticle. Of this the ends of our fingers, toes, and the foot of the hoofed animals, are remarkable examples.

Whether this sense is more acute in water, I am not certain, but should imagine it is.

Of the Sense of Taste.

The tongue, which is the organ of taste, is also endowed with the sense of touch. It is likewise to be considered, in the greatest number of animals, as an instrument for mechanical purposes; but probably less so in this tribe than any other. However, even in these, it must have been formed with this view, since, merely as an organ of taste, it would only have required surface, yet is a projecting body endowed with motion. In some, it is better adapted for motion than in others; and I should suppose this to be requisite, on account of the difference in the mode of catching the food, and in the act of swallowing. It is most projecting in those with teeth, probably for the better
conducting

conducting the food, step by step, to the œsophagus; whereas, it does not seem so necessary to have such management of the tongue in those which have no teeth, and catch their food by merely opening the mouth, and swimming upon it, or by having their prey carried in by the water. In the Porpoise and Grampus it is firm in texture, composed of muscle and fat, being pointed and ferrated on its edges, like that of the Hog.

In the Spermaceti Whale the tongue was almost like a feather-bed. In the Piked Whale it was but gently raised, hardly having any lateral edges, and its tip projecting but little, yet, like every other tongue, composed of muscle and fat. The extent between the two jaw bones in this Whale was very considerable, taking in the whole width of the head or upper jaw, and of course including the whalebone. This extent of surface, between jaw and jaw, having but little projection of tongue, is almost flat from side to side, is extremely elastic when contracted, and throws the inner membrane into a vast number of very small folds, that run parallel to one another, but which are again thrown into a close serpentine course by the elasticity of the part in a contrary direction. From the tongue being capable of but little motion, there is only a small mass of muscle required; and from the thinness of the jaw bones, the distance between the lower surface of the mouth and external surface of the skin is but small; and this skin being ribbed, and very elastic, is capable of considerable distention, by which the cavity of the mouth can be enlarged.

The tongue of the large Whalebone Whale, I should suppose, rose in the mouth considerably; the two jaws at the middle being kept at such a distance on account of the whalebone, so that the space between, when the mouth is shut, must be filled up by the tongue.

Of the Sense of Smelling.

In this tribe of animals there is something very remarkable in what relates to the sense of smelling; nor have I been able to discover the particular mode by which it is performed.

When we consider these animals as quadrupeds, and only constructed differently in external form for progressive motion through water, we must see that it was necessary that all the senses should correspond with this medium: we must therefore be at a loss to conceive how they smell, since we may observe, that the organ for smelling water, as in fish, is very different from that formed to smell air; and as we must suppose this tribe are only to smell water, being the medium in which such odoriferous particles can be diffused, we should expect their organ to be similar to that of fish; but in that case nature would have been obliged to have attached the nose of a fish to an animal constructed like a quadruped; and it is contrary to the laws which are established in the animal creation to mix parts of different animals together.

In many of this tribe there is no organ of smell at all; and in those which have such an organ, it is not that of a fish, therefore probably not calculated to smell water. It becomes difficult, therefore, to account for the manner in which such animals smell the water; and why the others should not have had such an organ *,

* Is the mode of smelling in fish similar to tasting in other animals? Or is the air contained in the water impregnated with the odoriferous parts, and this air the fish smells? If so, it is somewhat similar to the breathing of fish, it not being the water which produces the effect there, but the air contained in it. This I proved by experiments, and is mentioned by Dr. PRIESTLEY.

which,

which, I believe, is peculiar to the large and small Whalebone Whales.

Although it is not the external air which they inspire that produces smell, I believe it is the air retained in the nostril out of the current of respiration, which by being impregnated with the odoriferous particles contained in the water during the act of blowing, is applied to the organ of smell. It might be supposed, that they could smell the air on the surface of the water by every inspiration, as animals do on land; and probably they may: but this will not give them the power to smell the odoriferous particles of their prey in the water at any depth; and as their organ is not fitted to be affected by the application of water, and as they cannot suck water into the nostril, without the danger of its passing into the lungs, it cannot be by its application to this organ that they are enabled to smell.

Some have the power of throwing the water from the mouth through the nostril, and with such force as to raise it thirty feet high: this must answer some important purpose, although not immediately evident to us.

As the organ appears to be formed to smell air only, and as I conceive the smelling of the external air could not be of use as a sense. I therefore believe, that they do not smell in inspiration; yet let us consider how they may be supposed to smell the odoriferous particles of the water.

The organ of smell is out of the direct road of the current of air in inspiration; it is also out of the current of water when they spout; may we not suppose then, that this sinus contains air, and as the water passes in the act of throwing it out, that it impregnates this reservoir of air, which immediately affects the sense of smell. This operation is probably performed in the time of expiration, because it is said that this
water.

water is sometimes very offensive; but all this I only give as conjecture.

If the above solution is just, then only those which have the organ of smell can spout, a fact worthy of enquiry.

The organ of smell would appear to be less necessary in these animals than in those which live in air, since some are wholly deprived of it; and the organ in those which have it is extremely small, when compared with that of other animals, as well as the nerve which is to receive the impression, as was observed above.

Of the Sense of Hearing.

The ear is constructed much upon the same principle as in the quadruped; but as it differs in several respects, which it is necessary to particularise, to convey a perfect idea of it the whole should be described. As this would exceed the limits of this Paper, I shall content myself with a general description, taking notice of those material points in which it differs from that of the quadruped.

This organ consists of the same parts as in the quadruped; an external opening, with a membrana tympani, an Eustachian tube, a tympanum with its processes, and the small bones. There is no external projection forming a funnel, but merely an external opening. We can easily assign a reason why there should be no projecting ear, as it would interfere with progressive motion; but the reason why it is not formed as in birds, is not so evident: whether the percussions of water could be collected into one point as air, I cannot say. The tympanum is constituted with irregularities, so much like those of an external ear, that it appears to have a similar

The external opening begins by a small hole, scarcely perceptible, situated on the side of the head a little behind the eye. It is much longer than in other animals, in consequence of the size of the head being so much increased beyond the cavity that contains the brain. It passes in a serpentine course, at first horizontally, then downwards, and afterwards horizontally again, to the membrana tympani, where it terminates. In its whole length it is composed of different cartilages, which are irregular and united together by cellular membrane, so as to admit of motion, and probably of lengthening or shortening, as the animal is more or less fat.

The bony part of the organ is not so much inclosed in the bones of the skull as in the quadruped, consisting commonly of a distinct bone or bones, closely attached to the skull, but in general readily to be separated from it; yet in some it sends off, from the posterior part, processes which unite with the skull. It varies in its shape, and is composed of the immediate organ and the tympanum.

The immediate organ is, in point of situation to that of the tympanum, superior and internal, as in the quadruped. The tympanum is open at the anterior end, where the Eustachian tube begins.

The Eustachian tube opens on the outside of the upper part of the fauces: in some higher in the nose than others; highest, I believe, in the Porpoise. From the cavity of the tympanum, where it is rather largest, it passes forwards and inwards, and near its termination appears very much fasciculated, as if glandular.

The Eustachian tube and tympanum communicate with several sinuses, which passing in various directions surround the bone of the ear. Some of these are cellular, similar to the cells of the mastoid process in the human subject, although not bony. There is a portion of this cellular structure of a

particular kind, being white, ligamentous, and each part rather rounded than having flat sides*. One of the sinuses passing out of the tympanum close to the membrana tympani, goes a little way in the same direction, and communicates with a number of cells.

The whole function of the Eustachian tube is perhaps not known; but it is evidently a duct from the cavity of the ear, or a passage for the mucus of these parts; the external opening having a particular form would incline us to believe, that something was conveyed to the tympanum.

The bony part of the organ is very hard and brittle, rendering it even difficult to be cut with a saw, without its chipping into pieces. That part which contains the immediate organ is by much the hardest, and has a very small portion of animal substance in it; for when steeped in an acid, what remains is very soft, almost like a jelly, and laminated. The bone is not only harder in its substance, but there is on the whole more solid bone than in the corresponding parts of quadrupeds, it being thick and massy.

The part containing the tympanum is a thin bone, coiled upon itself, attached by one end to the portion which contains the organ; and this attachment in some is by close contact only, as in the Narwhale; in others, the bones run into one another, as in the Bottle-nose and Piked Whales.

The concave side of the tympanum is turned towards the organ, its two edges being close to it; the outer is irregular, and in many only in contact, as in the Porpoise: while in

* These communications with the Eustachian tube may be compared to a large bag on the base of the skull of the Horse and Ass, which is a lateral swell of the membranous part of the tube, and when distended will contain nearly a quart.

others

others the union is by bony continuity, as in the Bottle-nose Whale, leaving a passage on which the membrana tympani is stretched, and another opening, which is the communication with the sinuses.

The surface of the bone containing the immediate organ opposite to the mouth of the tympanum is very irregular, having a number of eminences and cavities. The cavity of the tympanum is lined with a membrane, which also covers the small bones with their muscles, and appears to have a thin cuticle. This membrane renders the bones, muscles, tendons, &c. very obscure, which are seen distinctly when that is removed. It appears to be a continuation of the periosteum, and the only uniting substance between the small bones. Besides the general lining, there is a plexus of vessels, which is thin and rather broad, and attached by one edge, the rest being loose in the cavity of the tympanum, somewhat like the plexus choroides in the ventricles of the brain. The cavity, we may suppose, intended to increase sound, probably by the vibration of the bone; and from its particular formation we can easily conceive, that the vibrations are conducted, or reflected, towards the immediate organ, it being in some degree a substitute for the external ear.

The external opening being smaller than in any animals of the same size, the membrana tympani is nearly in the same proportion. In the Bottle-nose Whale, the Grampus, and Porpoise, it is smooth and concave externally, but of a particular construction on the inner surface; for a tendinous process passes from it towards the malleus, converging as it proceeds from the membrane, and becoming thinner till its insertion into that bone. I could not discover whether it had any muscular fibres which could affect the action of the malleus. In

the Piked Whale, the termination of the external opening, instead of being smooth and concave, is projecting, and returns back into the meatus for above an inch in length, is firm in texture, with thick coats, is hollow on its inside, and its mouth communicating with the tympanum; one side being fixed to the malleus, similar to the tendinous process which goes from the inside of the membrana tympani in the others.

A little way within the membrana tympani, are placed the small bones, which are three in number, as in the quadruped, Malleus, Incus, and Stapes; but in the Bottle-nose Whale there is a fourth, placed on the tendon of the Stapedæus muscle. These bones are as it were suspended between the bone of the tympanum, and that of the immediate organ.

The malleus has two attachments, besides that with the incus; one close to the bone of the tympanum, which, in the Porpoise, is only by contact, but in others by a bony union; the other attachment is formed by the tendon, above described, being united to the inner surface of the membrana tympani. Its base articulates with the incus.

The incus is attached by a small process to the tympanum, and is suspended between the malleus and stapes. The process by which it articulates with the stapes is bent towards that bone.

The stapes stands on the vestibulum, by a broad oval base. In many of this tribe, the opening from side to side of the stapes is so small as hardly to give the idea of a stirrup.

The muscles which move these bones are two in number, and tolerably strong. One arises from that projecting part of the tympanum which goes to form the Eustachian tube, and running backwards is inserted into a small depression on the anterior part of the malleus. The use of this muscle seems to

be to tighten the membrana tympani ; but in those which have the malleus anchylosed with the tympanum, we can hardly conjecture its use. The other has its origin from the inner surface of the tympanum, and passing backwards is inserted into the stapes by a tendon, in which I found a bone in the large Bottle-nose. This muscle gives the stapes a lateral motion. What particular use in hearing may be produced by the action of these muscles, I will not pretend to say ; but we must suppose, whatever motion is given to the bones must terminate in the movement of the stapes.

The immediate organ of hearing is contained in a round, bony process, and consists of the Cochlea and Semicircular Canals, which somewhat resemble the quadruped ; but, besides the two spiral turns of the cochlea, there is a third, which makes a ridge within that continued from the foramen rotundum, and follows the turns of the canal.

The cochlea is much larger, when compared with the semicircular canals, than in the human species and quadruped.

We may reckon two passages into the immediate organ of hearing, the foramen rotundum, and foramen ovale. They are at a greater distance than in the quadruped. The foramen rotundum is placed much more on the outer surface of the bone, and not in the cavity of the bony tympanum ; but may be said to communicate with the surrounding cellular part of the tympanum. The foramen rotundum, which is the beginning of one of these turns, appears to be only one end of a transverse groove, which is afterwards closed in the middle, forming a canal with the two ends open ; so that this foramen appears to have two beginnings ; but the other opening is probably only a passage for blood-vessels going to the cochlea.

From

From this foramen begins the inner turn of the cochlea, which is the largest, especially at its beginning; the other begins from the vestibulum. The cochlea is a spiral canal coiled within itself, and divided into two by a thin spiral bony plate, which is compleated in the recent subject, and forms two perfect canals.

In the recent subject, the foramen rotundum is lined with the membrane of the tympanum, which terminates in a blind end, forming a kind of membrana cochleæ. The other opening, in the recent subject, communicates with the spiral turn, beyond the membranous termination of the foramen rotundum.

The foramen ovale has a little projection inwards all round, on which the stapes stands: within this is the vestibulum, which is common to the other spiral turn of the cochleæ, and the semicircular canals; this canal of the cochlea passes out first in a direction contrary to its general course, but soon makes a turn into the spiral. It is round, and not merely a division of the cochlea into two by a septum, but has a membrane of its own, which is attached to the thin bony plate, and lines that part of the cochlea in such a manner as to retain its structure when the bone is removed. The cochlea in some compleats one turn and an half; in others, more. It is not a spiral on a plane, or cylinder, but on a cone.

I have already observed, that by looking in at the foramen rotundum, we see two small ridges; the uppermost is the swell of the canal from the vestibulum just described; the lower ridge, which is also a canal, may be observed just to pass along the foramen belonging to this canal, close to the septum between the two; a circumstance, I believe, peculiar to this tribe. Its beginning is close to the vestibulum, but does not open from it, and passes along the first described spiral

turn to its apex: when opened, it appears to be a canal full of small perforations, probably the passages of the branches from the auditory nerve.

This bony process has several perforations in it; one of them large, for the passage of the seventh pair of nerves. The size of the portio mollis, before its entrance into the organ, is very large, and bears no proportion to that which enters. The passage for this nerve is very wide, and seems to have an irregular blind conical, and somewhat spiral, termination; its being spiral arises from the closeness to the point of the cochlea.

In the terminating part there are a number of perforations into the cochlea, and one into the semicircular canals, which afford a passage to the different divisions of the auditory nerve. There is a considerable foramen in its anterior side near the bottom, for the passage of the portio dura, and which is continued backward to the cavity of the tympanum near the stapes, and emerges near the posterior and upper part of this bone.

Of the Organ of Seeing.

The eye in this tribe of animals is constructed upon nearly the same principle as that of quadrupeds, differing, however, in some circumstances; by which it is probably better adapted to see in the medium through which the light is to pass. It is upon the whole small for the size of the animal, which would lead to the supposition, that their locomotion is not great; for, I believe, animals that swim are in this respect similar to those that fly; and as this tribe come to the surface of the medium in which they live, they may be considered in the same view with birds which soar; and we find, birds that

that fly to great heights, and move through a considerable space, in search of food, have their eyes larger in proportion to their size.

The eyelids have but little motion, and do not consist of loose cellular membrane, as in quadrupeds, but rather of the common adipose membrane of the body; the connexion, however, of their circumference with the common integuments is loose, the cellular membrane being less loaded with oil, which allows of a slight fold being made upon the surrounding parts in opening the eyelids. This is not to an equal degree in them all, being less so in the Porpoise than in the Piked Whale.

The tunica conjunctiva, where it is reflected from the eyelid to the eyeball, is perforated all round by small orifices of the ducts of a circle of glandular bodies lying behind it.

The lachrymal gland is small; its use being supplied by those above-mentioned; and the secretion from them all, I believe, to be a mucus similar to what is found in the Turtle and Crocodile. There are neither puncta nor lachrymal duct, so that the secretion, whatever it be, is washed off into the water.

The muscles which open the eyelids are very strong: they take their origin from the head, round the optic nerve, which in some requires their being very long, and are so broad as almost to make one circular muscle round the whole of the interior straight muscles of the eye itself. They may be divided into four; a superior, an inferior, and one at each angle: as they pass outwards to the eyelids, they diverge and become broader, and are inserted into the inside of the eyelids almost equally all round. They may be termed the dilators of the eyelids; and, before they reach their insertion, give off the external straight muscles, which are small, and inserted into the sclerotic coat before the transverse axis of the eye: these

these may be named the elevator, depressor, adductor, and abductor, and may be dissected away from the others as distinct muscles. Besides these four going from the muscles of the eyelid to the eye itself, there are two which are larger, and inclose the optic nerve with the plexus. As these pass outwards they become broad, may in some be divided into four, and are inserted into the sclerotic coat, almost all round the eye, rather behind its transverse axis.

The two oblique muscles are very long; they pass through the muscles of the eyelids, are continued on to the globe of the eye, between the two sets of straight muscles, and at their insertions are very broad; a circumstance which gives great variation to the motion of the eye.

The sclerotic coat gives shape to the eye, both externally and internally, as in other animals; but the external shape and that of the internal cavity are very dissimilar, arising from the great difference in the thickness of this coat in different parts. The external figure is round, except that it is a little flattened forwards; but that of the cavity is far otherwise, being made up of sections of various circles, being a little lengthened from the inner side to the outer, a transverse section making a short ellipsis.

In the Piked Whale the long axis is two inches and three-quarters, the short axis two inches and one-eighth.

The posterior part of the cavity is a tolerably regular curve, answering to the difference in the two axes; but forwards, near the cornea, the sclerotic coat turns quickly in, to meet the cornea, which makes this part of the cavity extremely flat, and renders the distance between the anterior part of the sclerotic

rotic coat and the bottom of the eye not above an inch and a quarter.

In the Piked Whale the sclerotic coat, at its posterior part, is very thick: near the extreme of the short axis it was half an inch, and at the long axis one-eighth of an inch thick. In the Bottle-nose Whale, the extreme of the short axis was half an inch thick, and the extremes of the long axis about a quarter of an inch, or half the other.

The sclerotic coat becomes thinner as it approaches to its union with the cornea, where it is thin and soft. It is extremely firm in its texture, where thick, and from a transverse section would seem to be composed of tendinous fibres, intermixed with something like cartilage; in this section four passages for vessels remain open. This firmness of texture precludes all effect of the straight muscles on the globe of the eye, by altering its shape, and adapting its focus to different distances of objects, as has been supposed to be the case in the human eye.

The cornea makes rather a longer ellipsis than the ball of the eye; the sides of which are not equally curved, the upper being most considerably so. It is a segment of a circle somewhat smaller than that of the eyeball, is soft and very flaccid.

The tunica choroides resembles that of the quadruped; and its inner surface is of a silver hue, without any nigrum pigmentum.

The nigrum pigmentum only covers the ciliary processes, and lines the inside of the iris.

The retina appears to be nearly similar to that of the quadruped.

The arteries going to the coats of the eye form a plexus passing round the optic nerve, resembling, in its appearance, that of the spermatic artery in the Bull and some other animals.

The

The crystalline humor resembles that of the quadruped; but whether it is very convex or flattened, I cannot determine, those I have examined having been kept too long to preserve their exact shape and size. The vitreous humor adhered to the retina at the entrance of the optic nerve.

The optic nerve is very long in some species, owing to the vast width of the head.

I shall not at present consider the eye in animals of this tribe, as it respects the power of vision, that being performed on a general principle common to every animal inhabiting the water; more especially as I am only master of the construction and formation of the eye, and not of the size, shape, and densities of the humors; yet, from reasoning, we must suppose them to correspond with the shape of the eye, and the medium through which the light is to pass.

Of the Parts of Generation.

The parts of generation in both sexes of this order of animals come nearer in form to those of the ruminating than of any others; and this similarity is, perhaps, more remarkable in the female than in the male; for their situation in the male must vary on account of external form, as was before observed.

The testicles retain the situation in which they were formed, as in those quadrupeds in which they never come down into the scrotum. They are situated near the lower part of the abdomen, one on each side, upon the two great depressors of the tail. At this part of the abdomen, the testicles come in contact with the abdominal muscles anteriorly.

The vasa deferentia pass directly from the epididymis behind the bladder, or between it and the rectum, into the urethra;

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and

and there are no bags fimilar to thofe called *vesiculæ feminales* in certain other animals.

The ftructure of the penis is nearly the fame in them all, and formed much upon the principle of the quadruped. It is made up of two *crura*, uniting into one *corpus cavernofum*, and the *corpus fpongiofum* feems firft to enter the *corpus cavernofum*. In the Porpoife, at leaft, the urethra is found nearly in the center of the *corpus cavernofum*; but towards the glans feems to feparate or emerge from it, and becoming a diftinct fpongy body, runs along its under furface, as in quadrupeds. The *corpus cavernofum* in fome is broader from the upper part to the lower than from fide to fide; but in the Porpoife has the appearance of being round, becoming fmaller forwards, fo as to terminate almoft in a point fome diftance from the end of the penis. The glans does not fpread out as in many quadrupeds, but feems to be merely a plexus of veins covering the anterior end of the penis, yet is extended a good way further on, and is in fome no more than one vein deep.

The *crura penis* are attached to two bones, which are nearly in the fame fituation and in the fame part of the pelvis as thofe to which the penis is attached in quadrupeds; but thefe bones are only for the infertion of the *crura*, and not for the fupport of any other part, like the pelvis in thofe animals which have pofterior extremities, neither do they meet at the fore part, or join the *vertebræ* of the back.

The *erectores penis* are very ftrong mufcles, having an origin and infertion fimilar to thofe of the human fubject.

The *acceleratores mufcles* are likewise very ftrong; and there is a ftrong and long mufcle, arifing from the anus, and paffing forwards to the bulb of the penis, that runs along the under furface of the urethra, and is at laft loft or inferted in the cor-

pus

pus spongiosum. This muscle draws the penis into the prepuce, and throws that part of the penis that is behind its insertion into a serpentine form. It is common to most animals that draw back the penis into what is called the sheath, and may be called the retractor penis.

In all the females which I have examined, the parts of generation are very uniformly the same; consisting of the external opening, the vagina, the two horns of the uterus, Fallopian tubes, fimbriæ, and ovaria.

The external opening is a longitudinal slit, or oblong opening, whose edges meet in two opposite points, and the sides are rounded off, so as to form a kind of sulcus. The skin and parts on each side of this sulcus are of a looser texture than on the common surface of the animal, not being loaded with oil, and allowing of such motion of one part on another as admits of dilatation and contraction. The vagina passes upwards and backwards towards the loins, so that its direction is diagonal respecting the cavity of the abdomen, and then divides into the two horns, one on each side of the loins; these afterwards terminating in the Fallopian tubes, to which the ovaria are attached. From each ovarium there is a small fold of the peritoneum, which passes up towards the kidney of the same side, as in most quadrupeds.

The inside of the vagina is smooth for about one-half of its length, and then begins to form something similar to valves projecting towards the mouth of the vagina, each like an ostium; these are about six, seven, eight, or nine in number. Where they begin to form, they hardly go quite round, but the last are complete circles. At this part too the vagina becomes smaller, and gradually decreases in width to its termination. From the last projecting part, the passage is continued

nued up to the opening of the two horns, and the inner surface of this last part is thrown into longitudinal rugæ, which are continued into the horns. Whether this last part is to be reckoned common uterus or vagina, and that the last valvular part is to be considered as os tincæ, I do not know; but from its having the longitudinal rugæ, I am inclined to think it is uterus, this structure appearing to be intended for distinction.

The horns are an equal division of this part; they make a gentle turn outwards, and are of considerable length. Their inner surface is thrown into longitudinal rugæ, without any small protuberances for the cotyledons to form upon, as in those of ruminating animals; and where they terminate, the Fallopian tubes begin.

In the Bottle-nose Whale, where the Fallopian tubes opened into the horns of the uterus, they were surrounded by pendulous bodies hanging loose in the horns.

The Fallopian tubes, at their termination in the uterus, are remarkably small for some inches, and then begin to dilate rather suddenly; and the nearer to the mouth the more this dilatation increases, like the mouth of a French horn, the termination of which is five or six inches in diameter. They are very full of longitudinal rugæ through their whole length.

The ovaria are oblong bodies, about five inches in length; one end attached to the mouth of the Fallopian tube, and the other near to the horn of the uterus. They are irregular on their external surface, resembling a capsula renalis or pancreas. They have no capsula, but what is formed by the long Fallopian tube.

How the male and female copulate, I do not know; but it is alledged, that their position in the water is erect at that time, which I can readily suppose may be true; for otherwise,
if

if the connexion is long, it would interfere with the act of respiration, as in any other position the upper surface of the heads of both could not be at the surface of the water at the same time. However, as in the parts of generation they most resemble those of the ruminating kind, it is possible they may likewise resemble them in the duration of the act of copulation; for, I believe, all the ruminants are quick in this act.

Of their uterine gestation I as yet know nothing; but it is very probable, that they have only a single young one at a time, there being only two nipples. This seemed to be the case with the Bottle-nose Whale, caught near Berkeley, which had been seen for some days with one young one following it, and they were both caught together.

The glands for the secretion of milk are two; one on each side of the middle line of the belly at its lower part. The posterior ends, from which go out the nipples, are on each side of the opening of the vagina, in small fulci. They are flat bodies lying between the external layer of fat and abdominal muscles, and are of considerable length, but only one-fourth of that in breadth. They are thin, that they may not vary the external shape of the animal, and have a principal duct, running in the middle through the whole length of the gland, and collecting the smaller lateral ducts, which are made up of those still smaller. Some of these lateral branches enter the common trunk in the direction of the milk's passage, others in the contrary direction, especially those nearest to the termination of the trunk in the nipple. The trunk is large, and appears to serve as a reservoir for the milk, and terminates externally in a projection, which is the nipple. The lateral portions of the sulcus which incloses the nipple, are composed of parts looser in texture than the common adipose membrane, which is probably to admit of the elongation or projection of
the

the nipple. On the outside of this there is another small fissure, which, I imagine, is likewise intended to give greater facility to the movements of all these parts.

The milk is probably very rich; for in that caught near Berkeley with its young one, the milk, which was tasted by Mr. JENNER and Mr. LUDLOW, Surgeon, at Sodbury, was rich like Cow's milk to which cream had been added.

The mode in which these animals must suck would appear to be very inconvenient for respiration, as either the mother or young one will be prevented from breathing at the time, their nostrils being in opposite directions, therefore the nose of one must be under water, and the time of sucking can only be between each respiration. The act of sucking must likewise be different from that of land animals; as in them it is performed by the lungs drawing the air from the mouth backwards into themselves, which the fluid follows, by being forced into the mouth from the pressure of the external air on its surface; but in this tribe, the lungs having no connexion with the mouth, sucking must be performed by some action of the mouth itself, and by its having the power of expansion.

E X P L A

EXPLANATION OF THE PLATES.

P L A T E XVI.

This fish is called a Grampus : it was caught in the mouth of the river Thames, in the year 1759, and brought up to Westminster Bridge in a barge. It was twenty-four feet long.

P L A T E XVII.

Another species of Grampus, which was caught in the river Thames, fifteen years ago. It was eighteen feet long.

P L A T E XVIII.

Fig. 1. A species of Bottle-nose Whale; the *Delphinus Delphis* of LINNÆUS. It was caught upon the sea-coast, near Berkeley, where it had been seen for several days, following its mother, and was taken along with the old one, and sent up to me whole, for examination, by Mr. JENNER, Surgeon, at Berkeley. The old one was eleven feet long.

Fig. 2. The head of the same Whale as fig. 1. to shew the shape of the blow-hole, which is transverse, and almost semi-circular.

P L A T E XIX.

The Bottle-nose Whale described by DALE. It is similar to that of Plate XVIII. in its general form, but has only two small pointed teeth in the fore part of the upper jaw, and is rather lighter coloured on the belly. It was caught above

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London Bridge in the year 1783, and became the property of the late Mr. Alderman PUGH, who very politely allowed me to examine its structure, and to take away the bones. It was twenty-one feet long.

PLATE XX.

Fig. 1. The *Balæna Rostrata* of FABRICIUS, or Piked Whale. It was caught upon the Dogger Bank. It had met with some accident between the two lower jaws under the tongue, in which part a considerable collection of air had taken place, so as to raise up the tongue and its attachments into a round body in the mouth, projecting even beyond the jaws. This rendered the head specifically lighter than the water, so that it could not sink, and therefore was easily caught.

It was seventeen feet long, and was brought to St. George's Fields, where I purchased it. The dorsal fin having been cut off close to the back, is therefore only marked by a dotted line.

Fig. 2. A view of the tail, to shew its breadth.

PLATE XXI.

Includes the external parts of generation, with the relative situation of the anus and the nipples, of the *Balæna Rostrata*.

Fig. 1. The labia pudendi spread open, exposing the meatus urinarius, vagina, and anus, which in a natural state are all concealed, there only appearing a long slit, the two edges of which are in contact.

AA. The labia pudendi.

B. The clitoris.

C. The meatus urinarius.

D. The

D. The opening of the vagina.

E. The anus.

Fig. 2. The fulcus, in which the left nipple lies, spread open, and the nipple itself exposed to view.

Fig. 3. The fulcus of the right nipple, in a natural state, only appearing like a line.

Fig. 4. A fulcus near to the nipple, which is spread open to shew the inside. This fulcus, I conceive, gives a freedom to the motion of the skin of these parts, so as to allow the nipple to be more freely exposed.

Fig. 5. The same fulcus on the opposite side, closed up.

P L A T E XXII.

A side view of one of the plates of whalebone of the *Balaena Rostrata*.

A. The part of the plate which projects beyond the gum.

B. The portion which is sunk into the gum.

CC. A white substance, which surrounds the whalebone, forming there a projecting bead, and also passing between the plates, to form their external lamellæ.

DD. The part analogous to the gum.

E. A fleshy substance, covering the jaw bone, and on which the inner lamella of the plate is formed.

F. The termination of the plate of whalebone in a kind of hair.

P L A T E XXIII.

Fig. 1. A perpendicular section of several plates of whalebone in their natural situation in the gum; their inner edges, or shortest terminations, are removed, and the cut edges of the plates seen from the inside of the mouth.

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The upper part shews the rough surface formed by the hairy termination of each plate of whalebone.

The middle part shews the distance the plates of whalebone are from each other.

The lower part shews the white substance in which they grow, and also the basis on which they stand.

Fig. 2. An outline considerably magnified, to shew the mode of growth of the plates, and of the white intermediate substance.

A. The middle layer of the plate, which is formed upon a pulp or cone that passes up in the centre of the plate. The termination of this layer forms the hair.

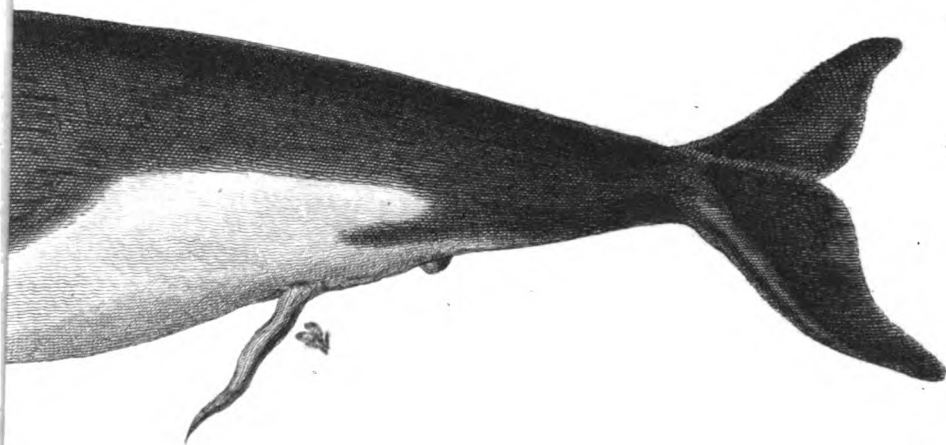
B. One of the outer layers, which grows, or is formed, from the intermediate white substance.

CCCC. The intermediate white substance, laminæ of which are continued along the middle layer, and form the substance of the plate of whalebone.

D. The outline of another plate of whalebone.

E. The basis on which the plates are formed, which adheres to the jaw bone.





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Fig. 1.

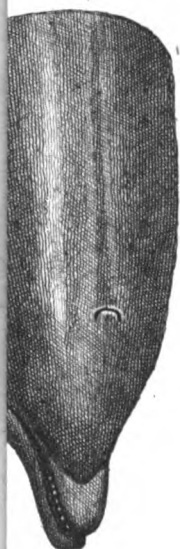
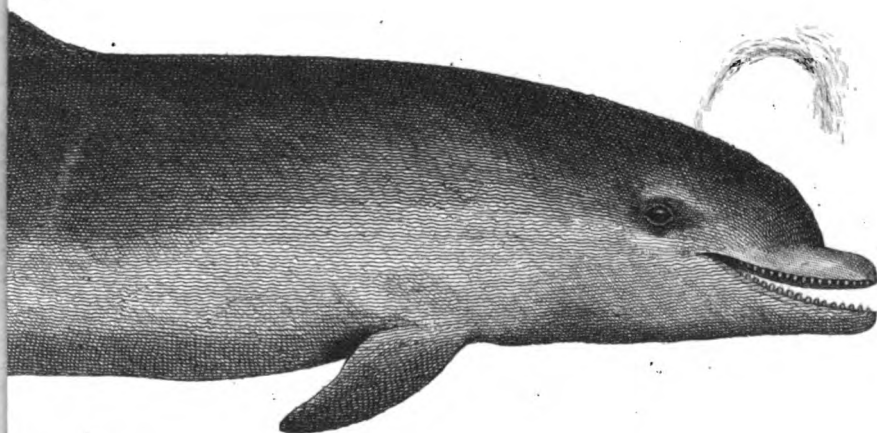
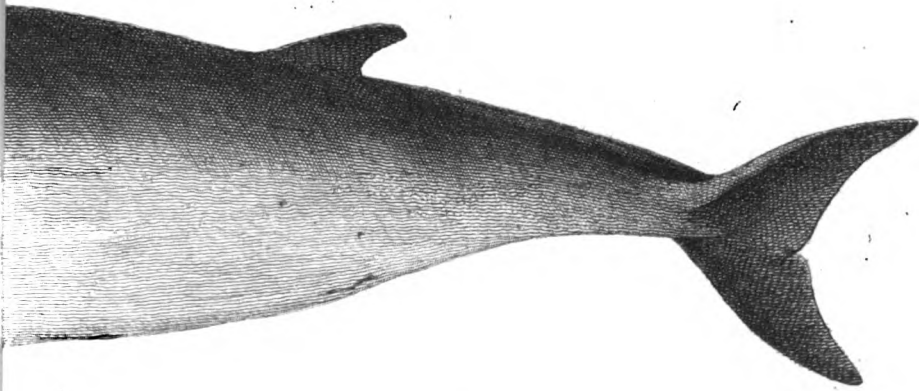


Fig. 2.

Bayle sculp.



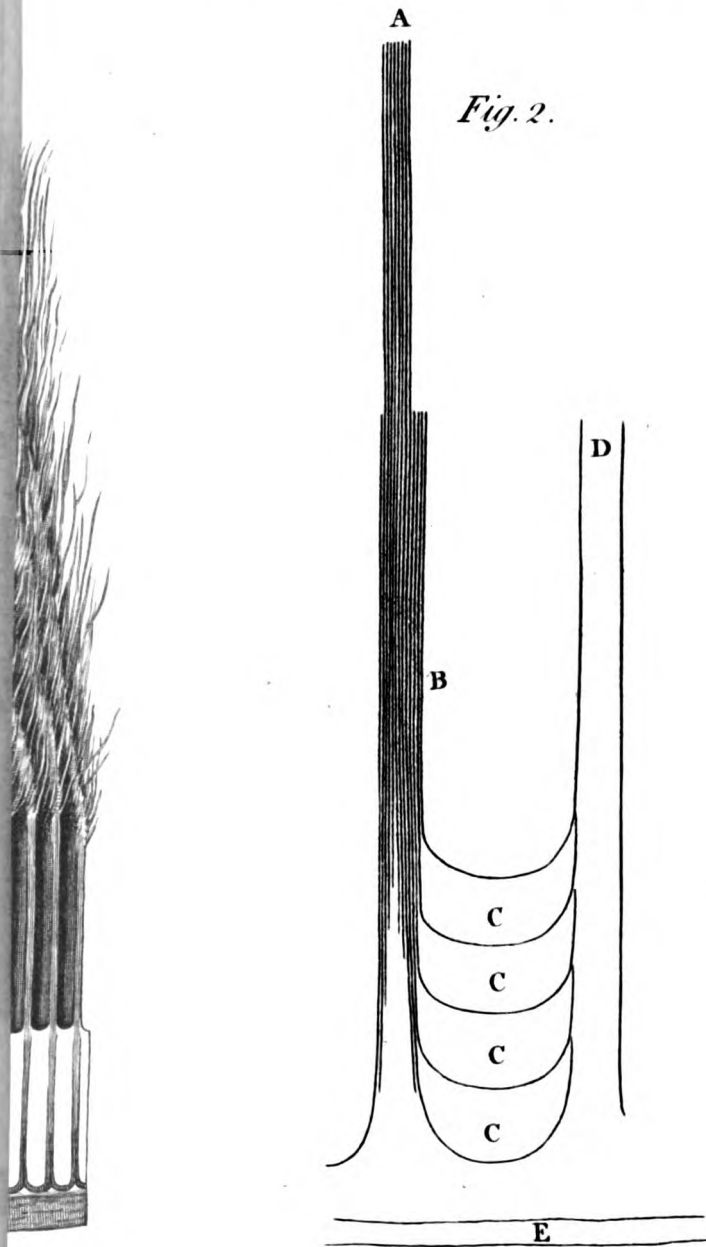
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XXXIX. *Some Observations on ancient Inks, with the Proposal of a new Method of recovering the Legibility of decayed Writings.* By Charles Blagden, M. D. Sec. R. S. and F. A. S.

Read June 28, 1787.

IN a conversation some time ago with my friend THOMAS ASTLE, Esq. F. R. S. and A. S. relative to the legibility of ancient MSS. a question arose, whether the inks in use eight or ten centuries ago, and which are often found to have preserved their colour remarkably well, were made of different materials from those employed in later times, of which many are already become so pale as scarcely to be read. With a view to the decision of this question, Mr. ASTLE obligingly furnished me with several MSS. on parchment and vellum, from the ninth to the fifteenth centuries inclusively; some of which were still very black, and others of different shades of colour, from a deep yellowish brown to a very pale yellow, in some parts so faint as to be scarcely visible. On all of these I made experiments with the chemical re-agents which appeared to me best adapted to the purpose; namely, alkalies both simple and phlogisticated, the mineral acids, and infusion of galls.

It would be tedious and superfluous to enter into a detail of the particular experiments; as all of them, one instance only excepted, agreed in the general result, to shew, that the ink employed anciently, as far as the above-mentioned

MSS. extended, was of the same nature as the present: for the letters turned of a reddish or yellowish brown with alkalis, became pale, and were at length obliterated, with the dilute mineral acids, and the drop of acid liquor which had extracted a letter, changed to a deep blue or green on the addition of a drop of phlogisticated alkali; moreover, the letters acquired a deeper tinge with the infusion of galls, in some cases more, in others less. Hence it is evident, that one of the ingredients was iron, which there is no reason to doubt was joined with the vitriolic acid; and the colour of the more perfect MSS. which in some was a deep black, and in others a purplish black, together with the restitution of that colour, in those which had lost it, by the infusion of galls, sufficiently proved that another of the ingredients was astringent matter, which from history appears to have been that of galls. No trace of a black pigment of any sort was discovered, the drop of acid, which had completely extracted a letter, appearing of an uniform pale ferrugineous colour, without an atom of black powder, or other extraneous matter, floating in it.

As to the greater durability of the more ancient inks, it seemed, from what occurred to me in these experiments, to depend very much on a better preparation of the material upon which the writing was made, namely, the parchment or vellum; the blackest letters being generally those which had sunk into it the deepest. Some degree of effervescence was commonly to be perceived when the acids came in contact with the surface of these old vellums. I was led, however, to suspect, that the ancient inks contained a rather less proportion of iron than the more modern; for in general the tinge of colour, produced by the phlogisticated alkali in the acid laid upon them, seemed less deep; which, however, might depend in part upon the
6 length

length of time they had been kept: and perhaps more gum was used in them, or possibly they were washed over with some kind of varnish, though not such as gave any gloss.

One of the specimens sent me by Mr. ASTLE proved very different from the rest. It was said to be a MS. of the fifteenth century; and the letters were those of a full engrossing hand, angular, without any *fine* strokes, broad, and very black. On this none of the above-mentioned re-agents produced any considerable effect; most of them rather seemed to make the letters blacker, probably by cleaning the surface; and the acids, after having been rubbed strongly upon the letters, did not strike any deeper tinge with the phlogisticated alkali. Nothing had a sensible effect toward obliterating these letters but what took off part of the surface of the vellum; when small rolls, as of a dirty matter, were to be perceived. It is therefore unquestionable, that no iron was used in this ink; and from its resistance to the chemical solvents, as well as a certain clotted appearance in the letters when examined closely, and in some places a slight degree of gloss, I have little doubt but they were formed with a composition of a black sooty or carbonaceous powder and oil, probably something like our present Printer's ink, and am not without suspicion that they were actually printed*.

Whilst I was considering of the experiments to be made, in order to ascertain the composition of ancient inks, it occurred to me, that perhaps one of the best methods of restoring legibility to decayed writing might be, to join phlogisticated alkali with the remaining calx of iron; because, as the quantity of pre-

* A subsequent examination of a larger portion of this supposed MS. has shewn, that it is really part of a very ancient printed book.

precipitate formed by these two substances very much exceeds that of the iron alone, the bulk of colouring matter would thereby be greatly augmented. M. BERGMAN was of opinion, that the blue precipitate contains only between a fifth and a sixth part of its weight of iron; and though subsequent experiments * tend to shew that, in some cases at least, the proportion of iron is much greater, yet upon the whole it is certainly true, that if the iron left by the stroke of a pen were joined to the colouring matter of phlogisticated alkali, the quantity of Prussian blue thence resulting would be much greater than the quantity of black matter originally contained in the ink deposited by the pen; though perhaps the body of colour might not be equally augmented. To bring this idea to the test, I made a few experiments as follows.

The phlogisticated alkali was rubbed upon the bare writing, in different quantities; but in general with little effect. In a few instances, however, it gave a bluish tinge to the letters, and increased their intensity, probably where something of an acid nature had contributed to the diminution of their colour.

Reflecting that when the phlogisticated alkali forms its blue precipitate with iron, the metal is usually first dissolved in an acid, I was next induced to try the effect of adding a dilute mineral acid to writing, besides the alkali. This answered fully to my expectations; the letters changing very speedily to a deep blue colour, of great beauty and intensity. It seems of little consequence as to the strength of colour obtained, whether the writing be first wetted with the acid, and then the phlogisticated alkali be touched upon it, or whether the process be inverted, beginning with the alkali; but on another account, I think, the

* CRELL *Eyträge*, B. I. ft. I. p. 42, &c.

latter

latter way preferable. For the principal inconvenience which occurs in the proposed method of restoring MSS. is, that the colour frequently spreads, and so much blots the parchment, as to detract greatly from the legibility; now this appears to happen in a less degree when the alkali is put on first, and the dilute acid is added upon it. The method I have hitherto found to answer best has been, to spread the alkali thin with a feather over the traces of the letters, and then to touch it gently, as nearly upon or over the letters as can be done, with the diluted acid, by means of a feather, or a bit of stick cut to a blunt point. Though the alkali has occasioned no sensible change of colour, yet the moment that the acid comes upon it, every trace of a letter turns at once to a fine blue*, which soon acquires its full intensity, and is beyond comparison stronger than the colour of the original trace had been. If now the corner of a bit of blotting paper be carefully and dexterously applied near the letters, so as to suck up the superfluous liquor, the staining of the parchment may be in great measure avoided: for it is this superfluous liquor, which, absorbing part of the colouring matter from the letters, becomes a dye to whatever it touches. Care must be taken not

* The *phlogisticated alkali* (which is to be considered simply as a name) appears to consist of a peculiar acid, in the present extensive acceptation of that term, joined to the alkali. Now the theory of the above-mentioned process I take to be, that the mineral acid, by its stronger attraction for the alkali, dislodges the colouring (Prussian) acid, which then immediately seizes on the calx of iron, and converts it into Prussian blue, without moving it from its place. But if the mineral acid be put upon the writing first, the calx of iron is partly dissolved and diffused by that liquor before the Prussian acid combines with it; whence the edges of the letters are rendered more indistinct, and the parchment is more tinged. The sudden evolution of so a fine colour, upon the mere traces of letters, affords an amusing spectacle.

to bring the blotting paper in contact with the letters, because the colouring matter is soft whilst wet, and may easily be rubbed off.

The acid I have chiefly employed has been the marine; but both the vitriolic and nitrous succeed very well. They should undoubtedly be so far diluted as not to be in danger of corroding the parchment, after which the degree of strength does not seem to be a matter of much nicety.

The method now commonly practised to restore old writings, is by wetting them with an infusion of galls in white wine*. This certainly has a great effect; but it is subject, in some degree, to the same inconvenience as the phlogisticated alkali, of staining the substance on which the writing was made. Perhaps if, instead of galls themselves, the peculiar acid or other matter which strikes the black with iron were separated from the simple astringent matter, for which purpose two different processes are given by PIEPENBRING† and by SCHEELÉ‡, this inconvenience might be avoided. It is not improbable likewise, that a phlogisticated alkali might be prepared, better suited to this object than the common; as by rendering it as free as possible from iron, diluting it to a certain degree, or substituting the volatile alkali for the fixed. Experiment would most likely point out many other means of improving the process described above; but in its present state I

* See a complicated process for the preparation of such a liquor in CANEPARIUS, *De Atramentis*, p. 277.

† CRELL. *Annal.* 1786, B. I. p. 51.

‡ Kongl. Vetensk. Acad. Nya Handlingar, tom. VII. p. 30. See also M. DE MORVEAU's account of this substance in the *Encyclopédie par ordre des matières*.

hope it may be of some use, as it not only brings out a prodigious body of colour upon letters which were before so pale as to be almost invisible, but has the further advantages over the infusion of galls, that it produces its effect immediately, and can be confined to those letters only for which such assistance is wanted.



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- Aufmunterung zu sorgfältiger Mit- forschung der Verhältnisse, welche die Gewächarten bey ihrer Vegeta- tion gegen einander beobachten, von F. A. L. von Burgdorf. 1785. 4° M. von Burgdorf.
- Die Tegelfche Baumzucht in einem Grundverzeichnisse, durch den Forst- rath von Burgdorf. Fol. pat.
- Dec. 7. Idées sur la Météorologie, par J. A. De Luc. Tome I. 2 Vol. Londres, 1786. 8° M. De Luc, F.R.S.
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The London Medical Journal. Vol. VII. London, 1786. 8°

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A Meteorological Journal of the Year 1786, kept in Fater-Noster-Row, London, by W. Bert. 4°

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 April 19. *Expériences sur les Végétaux*, par J. Ingenhoufz. Paris, 1787. 8°
A Treatise on Magnetism, by T. Cavallo. London, 1787. 8°
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 14. Medical Remarks on natural, spontaneous, and artificial Evacuation, by J. Anderson. London, 1787. 8°
 21. Tableau des Variétés de la Vie Humaine, par M. G. Daignan. Paris, 1786. 2 Vol. 8°
 28. Meteorological Journal kept at Odiham, in Hampshire, from April 1, 1786, to April 1, 1787, by A. Baxter, MS. fol.
 Meteorological Journal kept at Bencoolen, from Sept. 1. to Nov. 30, 1784, by John Macdonald, MS. 4°
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A P P E N D I X.

A Supplement to Major-General Roy's Account of the Mode proposed to be followed in determining the relative Situation of the Royal Observatories of Greenwich and Paris. See p. 188.

Read Nov. 8, 1787.

IN the account of the proposed trigonometrical operation for determining the difference between the meridians of the Royal Observatories of Greenwich and Paris, I have, at p. 221. and 222. had occasion to remark on an inconsistency found in the sum of the three equations, as stated by M. BOUGUER, for obtaining the lengths of degrees of great circles perpendicular to the meridian, above their corresponding degrees of latitude, without having been aware of the true source of the error, for which I am indebted to the investigations of the Astronomer Royal; who, having found it out, obligingly communicated the same to me, about the time of the annual recess of the Society in the end of June last.

At p. 289. and again at p. 313. and 314. of M. BOUGUER's Book, the subtractive branch of the equation, or the part of DG the gravicentric arc, answering to the difference between the *radii* of curvature at the equator and given latitude, has erroneously been expressed in words $\frac{1}{4}$ ths instead of $\frac{3}{4}$ ths, which the algebraic *formula* justly gives for it, and according to which M. BOUGUER's table has been accurately computed. Not suspecting any thing of this sort, no intima-

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tion

tion of it appearing among the *errata*, and no notice having been hitherto taken, as far as I know, of such a mistake existing in that justly celebrated work, now so many years in circulation throughout the world; instead therefore of $\frac{4}{3}$ ths, or $\frac{80}{100}$ parts of the third proportional, the last additive member of the equation, I substituted another (with a certain modification, however, as stated in the before-mentioned pages) amounting only to $\frac{80}{170}$ parts of the third proportional, being the highest that would apply to the whole quadrant, without producing absurd results. Thus I obtained approximate degrees of great circles and of longitude, differing but little from those of M. BOUGUER, and compensating in a great measure, although not altogether, for the then undiscovered cause of the mistake of $\frac{1}{2}$ th part of the arc DG; for $\frac{4}{3} - \frac{1}{2} = \frac{5}{6}$.

In this state of the case, I have judged it incumbent on me to annex a supplementary table, where the degrees of great circles and of longitude are accurately computed by the corrected subtractive branch $\frac{4}{3}$ ths of DG instead of $\frac{1}{2}$ ths, as it now stands in the original table. From inspection it will appear, that the maximum of correction amounts nearly to $5\frac{1}{2}$ fathoms at the 70th degree of latitude, diminishing gradually from thence to the pole on one side, and the equator on the other, where it vanishes. The maximum of correction for the degrees of longitude, amounting to about $2\frac{1}{2}$ fathoms, is applicable between the 58th and 59th degree of latitude, where M. BOUGUER's degree of the meridian becomes equal to his degree of longitude on the equator. From this point, it diminishes gradually to the pole on one side, and the equator on the other, where it in like manner disappears.

With regard to degrees of great circles situated obliquely to the meridian, it is sufficiently obvious, that they are so little affected as to render it of but small importance whether they

are corrected or not ; but for such as are scrupulous to fractional parts of fathoms, these may satisfy themselves with great facility and exactness, by attending to and proportioning by the common tabular differences, as in the following example of the application of the correction to oblique degrees, in the latitude of Greenwich $51^{\circ} 28' 40''$.

	Obliquity.	Original tabular difference.	Supp. correc- tion.	Corrected difference.	Corrected degree.
		Fath.	Fath.	Fath.	Fath.
Application of the correction to the degrees of great circles, situated obliquely to the meridian, in the latitude of Green- wich,	$45^{\circ} 0'$	204.45	-1.65	+ 202.80	61070.20
	$33^{\circ} 45'$	78.24	-0.63	$\left\{ \begin{array}{l} - \\ + \end{array} \right\}$ 77.61	$\left\{ \begin{array}{l} 60992.63 \\ 61147.85 \end{array} \right\}$
	$56^{\circ} 15'$			$\left\{ \begin{array}{l} - \\ + \end{array} \right\}$ 65.79	$\left\{ \begin{array}{l} 60926.84 \\ 61213.64 \end{array} \right\}$
	$22^{\circ} 30'$	66.33	-0.54	$\left\{ \begin{array}{l} - \\ + \end{array} \right\}$ 43.96	$\left\{ \begin{array}{l} 60882.88 \\ 61257.60 \end{array} \right\}$
	$67^{\circ} 30'$			$\left\{ \begin{array}{l} - \\ + \end{array} \right\}$ 15.44	$\left\{ \begin{array}{l} 60867.44 \\ 61273.04 \end{array} \right\}$
	$11^{\circ} 15'$	44.32	-0.36		
	$78^{\circ} 45'$				
	$0^{\circ} 0'$	15.56	-0.12		
	$90^{\circ} 0'$				

Here it is to be observed, that half of the correction is constantly to be applied at the 45^{th} degree of obliquity ; and since the differences between the terms in the progression equally removed on either side from 45° , are always equal to each other, it follows, that the corrected differences are to be applied with the contrary sign, those between 45° and the meridian being in diminution, and these between 45° and the east or west points being in augmentation of the length, till, in the first case, the degree becomes equal to that of latitude ; and in the last to that of a great circle perpendicular to the meridian.

The Table of Comparison, p. 227. is no where affected by the alteration which has been the subject of this discussion, except in the two last lines from the bottom, as adverted to among the *errata* subjoined to this supplement.

This intimation of M. BOUGUER's mistake in expressing his formula in words, was accompanied with the conversion of
that

that formula by Dr. MASKELYNE into the following, adapted to natural sines, $\frac{a \times 11 + 8 \times \cos. 2 \text{ lat} - 3 \cos. 4 \text{ lat.}}{30}$. a is the excess of the radius of curvature at the pole above that at the equator, and the sign of either term is only to be changed when the doubled or quadrupled latitude become greater than 90° , and less than 270° .

I embrace this opportunity of mentioning another circumstance, wholly unknown to me at the time my Paper was composed. From what has been there said, at p. 216. and so on to p. 220. it will probably be inferred, that I considered the proposed mode of determining the differences of longitude by the observations of the pole star, made with a very accurate instrument, rather as new, not knowing that the same idea, or one nearly the same, had before occurred to the Rev. Mr. MICHELL, and been treated on by him in his very ingenious Paper in the Philosophical Transactions, Vol. LVI. for the year 1766. That I must have read that valuable performance about the time of its publication is not to be doubted; but in the lapse of so many years, every trace of it had gone from my remembrance, otherwise I would have most certainly referred to it in the proper place, and with the attention that it so well deserves. However, without entering here into particulars, it will obviously appear, that the one has not been borrowed from the other.

E R R A T A.

In p. 195. l. 27. for between Gravelines and Calais read between Watten and Gravelines. And in l. 28. for $56' 42' 0''$ read $46' 52' 0''$.

In p. 197. in the distance of the parallel of Rodés from that of Dunkirk, for $6^\circ 50' 51'' 14'''$ read $6^\circ 40' 51'' 14'''$.

In the last line of the note at the bottom of p. 217. instead of $1^\circ 49' 8''.8$, read $1^\circ 49' 48''.8$.

In p. 221. line 6. from the top, instead of $\frac{7}{13}$ ths, read $\frac{4}{13}$ ths.

In the Table of Comparison, p. 227. the last line but one of the last column but one, for 44373.5 read 44372.0. In the next line below, for 38164.0, as formerly corrected, read 38162.0. And in the right-hand column, last line but one from the bottom, for +19.1, read +17.6.

— 1875 —

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— 1892 —

— 1893 —

Translation of Father Joseph da Rovato's Letter to the Royal Society, relative to Borax. See p. 301.

THE Father prefect of the Mission in Thibet has the pleasure to acquaint the Royal Society, that, residing at Patna, he has frequently been desired by M. VOGLER, an able naturalist from Germany, to obtain some circumstantial account of the places where, and the manner in which, the borax procured from the kingdom of Thibet is obtained; no one else, as he said, having any communication with those almost impenetrable parts. Although our Mission have long since forsaken that kingdom, yet the Father prefect being somewhat connected with the *Bahadur Shah*, brother to the King of Nepal (whose kingdom extends northward as far as Kuti on the frontiers of Thibet), he wrote to him, and requested all the information that could be obtained on the subject. The Bahadur Shah, in order to give the best satisfaction in his power, was pleased to send to the prefect, as far as Patna, a man in his service, who, being a native of the country where the Borax is prepared, could give the most ample intelligence concerning that substance.

This man, partly in the Nepalese and partly in the Hindoo language, both which are understood by the prefect, gave the following account. In the province or territory of Marmé, twenty-eight days journey to the north of Nepal, and twenty-five to the West

X x x of

of Lassa, the capital of Thibet, there is a vale about eight miles broad. In a part of this vale there are two villages or castles, the one named *Scierugh*, and the other *Kanglé*, the inhabitants of which are wholly employed in digging the borax, which they sell into Thibet and Nepal, they having no other means of subsistence, the soil being so barren as to produce nothing but a few rushes. Near the two above-mentioned castles there is a pool of a moderate size, and some smaller ones, where the ground is hollow, in which the rain-water collects. In these pools, after the water has been some time detained in them, the borax is formed naturally: the men, wading into the water, feel a kind of a pavement under their feet, which is a sure indication that borax is there formed, and there they accordingly dig it.

Where there is little water, the layer of borax is thin; and where it is deep, it is thicker, and over the latter there is always an inch or two of soft mud, which is probably a deposit of the water, after it has been agitated by rain or wind. Thus is the borax produced merely by nature, without either boiling or distillation. The water in which it is formed is so bad, that the drinking a small quantity of it will occasion a swelling of the abdomen, and in a short time death itself. The earth that yields the borax is of a whitish colour; and in the same valley, about four miles from the pools, there are mines of salt, which is there dug in great abundance for the use of all the inhabitants of these mountains who live at a distance from the sea. The natives, who have no other subsistence on account of the sterility of the soil, pay nothing for digging borax; but strangers must pay a certain retribution, and usually agree at so much a workman. This is paid to a Lama, named Pema *Tupkan*, who owns the pits in Marmé.

Ten

Ten days journey farther north, there is another valley named *Tapré*, where they dig borax, and another still farther, called *Cioga*; but of this latter I have not marked the situation. Borax is in the Hindoo and Nepalese languages called *Soaga*. If it be not purified, it will easily deliquesce; and in order to preserve it any time, till they have an opportunity of selling it, the people often mix it with earth and butter.

In the territory of *Mungdan*, sixteen days journey to the north of Nepal, there are rich mines of arsenic; and in various other places are found mines of sulphur, as also of gold and silver, whose produce is much purer than those of the mines of Pegu.

This is the substance of the information obtained from the man sent by the Bahadur Shah. If the Gentlemen of the Royal Society wish to see any of the soil which yields the borax, it may be easily obtained, since the said Bahadur Shah, who now governs the whole of the province of Nepal, is well disposed towards the Father prefect, and will probably not refuse him the favour of sending a trusty person to gather some of the soil, and to send it down to Patna. The Father prefect will easily find an opportunity of sending it thence to London.

This is what the Father prefect takes the liberty to mention to the Royal Society. He, moreover, tenders his own best services, and those of the other Italian Capuchins, his brethren Missionaries, if they could communicate any other useful intelligence; they being very desirous to prove their gratitude to the English nation, from whom they have received, and are ever receiving, many and singular benefits.

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Page. Line.

- 249. 10. *for* "last to the" *read* "last term to ten times the"
- 254. 25. *for* "has" *read* "have"
- 301. 13. *for* "alto" *read* "altro"
- 302. 9. *for* "alto" *read* "altro"
- 305. 18. *for* "celle" *read* "celles"
- 20. *for* "at" *read* "et"
- 22. *for* "cet" *read* "cette"
- 330. 20. *for* "was" *read* "were"
- 336. 23. *for* "LC" *read* "Lc"
- 339. 14. *for* "2d" *read* "23d"
- last line. *for* "15 13 52.4" *read* "16 13 52.4"
- 340. 8. *for* "15 h. 13' 52''.4" *read* "16 h. 13' 52''.4"
- 416. 24. and 25. *for* "we do know" *read* "we do not know"
- 429. 22. *for* a full stop after "sense" *put* a comma

